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A PROBABILISTIC APPROACH TO LANGUAGE LEARNING[1]

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Experiments designed to study "concept formation," especially "language learning" do not usually resemble the actual situations in which concepts are formed and languages learned. For example, subjects in such experiments are usually required to learn the "names" of objects divided into "mutually exclusive" categories. In contrast, the child learning names of objects in his environment has no such ready-made, clear-cut divisions to guide him. Also the child is confronted with objects between which he can clearly discriminate but which nevertheless happen to be called by the same name. In this paper experiments are described in which the discriminability between objects is controlled by using as referents equally discriminable colors. The frequency with which members of referent categories are called by particular names is varied by the experiment, and the resulting effects of these variations as language learning are studied.

EXPERIMENTS on concept formation have occasionally been regarded as models for the acquisition of word-meanings. The subjects (*Ss*) are shown some stimulus material and are told that part of the material are "piffles" and part "puffles." The task is to discover the "meaning" of these nonsense words. These experiments differ in two important respects from the situation in which an infant who is learning to speak finds himself. First, such experiments usually operate with concepts which *S* has already learned previous to the experiment so that the task consists merely of substituting a nonsense word for an English word (e.g., "triangles" are "piffles"). But the infant can make no such substitution; his task is to learn to order the world in terms of a certain, hitherto unfamiliar criterion, namely triangularity. Second, the experiments introduce a simplification into this ordering task which is very unusual in the actual learning situation. The experimental stimulus material will usually be clearly divided into either "piffles" or "puffles" (triangles *or* no-triangles; animals *or* vegetables; angular things *or* round things, etc.) so that one clue toward the solution of the problem is simply to find the one criterion that divides the stimulus objects into two clear categories. Such a dichotomous principle will then completely determine "piffleness." This is a degree of determinacy which is hardly ever encountered by the child. An object that is called "table" by mother, may be called "desk" by father; the color of the new car may be called "chartreuse" by Mary and "green" by Joe; the woman across the street is known as a "mother" in one context and as a "lady" in another. To make the confusion more complete, the individual constituents within the classes "table," "green," and "lady" are all discriminably different from one another, and there seems to be no sharp distinction between objects that are "tables"

[1] This experiment is part of an investigation carried out in connection with a doctoral dissertation submitted to Harvard University in 1955. The sponsorship of and assistance from the Center for International Studies, M.I.T., the Social Science Research Council, and the Laboratory of Social Relations, Harvard University are gratefully acknowledged. The author is indebted to Roger W. Brown and J. S. Bruner for valuable advice.

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and objects that are "desks." Every class appears to blend into a number of other classes.

One of the two objectives of this study is to verify empirically the assertions made above. Thus, besides concepts already familiar to the *S* from earlier training, unfamiliar concepts were introduced in an experiment in such a way that the process of attainment of familiar concepts could be compared directly with the attainment of unfamiliar ones. Furthermore, the stimulus material differed from the material usually employed in the more traditional type of concept formation test in that it did not fall into perceptually clear-cut categories such as triangles, green hats, or four symbols; instead, graded and perceptually equidistant stimuli were used which made it possible to investigate empirically whether or not some specific conceptual categories (colors) actually do have this interblending character. The question was posed: is it true that a given physical color is in fact called "green" by 35% of the population, "blue" by 40%, and "aqua" by 25%? And if so, what is the nature of the frequency distribution of color words over a specific stimulus continuum?

The other objective of this study is the logical sequel to the first: if it is true that conceptual categories—at least in so far as they are reflected in *S*s' language habits—have widely overlapping frequency distributions, what is the role played by the *shapes* of these frequency distributions, particularly their slopes, in the learning of these concepts? How does an experimental change in the slope of the frequency distributions affect performance on a concept attainment task?

The present investigation, then, has affinities with the well-known concept formation tests as well as with probability learning experiments initiated by Egon Brunswick (4) but it differs from either in the following points:

1. The attainment of familiar concepts is compared with attainment of unfamiliar ones.

2. Perceptually graded stimulus material is used.

3. Concepts are best characterized as areas of waxing and waning typicality on a stimulus continuum.

4. The domain of any one concept is delimited by the domain of some other concept and thus the whole apparatus of conceptual categories is interdependent.

5. The *S* does not respond to or learn individual probabilities but learns to discern an entire pattern of frequency distributions.

6. The slope of frequency distributions is introduced as independent variable in a learning task.

7. The *S*s have no direct feedback thus reducing reinforcement to a minimum.

METHOD

Stimulus material. The stimulus material used was every fourth cap (beginning with number one) of the Farnsworth-Munsell 100 Hue Test for the Examination of Color Discrimination (2, 7). Thus a circular array of equidistant stimuli was obtained with easily discriminated steps and without scale ends. The colors were presented against a gray background of Munsell value five (18% to 22% reflectance) under a standard light source (fluorescent daylight).

Determination of English color terms for the stimulus material. First it was necessary to gather accurate data about English classification schemes with respect to the stimulus material. The variety of English names for these colors and also the probability or relative frequency of occurrence of such names had to be ascertained (1). Twenty-seven Wellesley College students were instructed to write down the "name" of every color. "By name, we mean the words you would use if you were to describe the color to another girl." These instructions were given orally and *S*s were encouraged to ask questions about the task, particularly if the experimenter (*E*) had the impression that the task had not yet been fully understood. The entire set of colors was shown to every *S* prior to instructions. Then the colors were turned upside down and presented randomly, a different order for every *S*. There was no time limit for the task. The frequency distributions of the names that occurred most commonly are

shown in Fig. 1. In plotting these curves, modifiers of the root names were disregarded. (E. g., green, light green, bluish green, yellow-green were all integrated into one curve called green in the graph; green-yellow was counted as yellow, since most English speakers agree that the last name in such a phrase is the root whereas preceding names serve as modifiers.) This procedure was based on the assumption that in colloquial discourse, especially where no stress is placed on accurate description, any of the phrases built around the root "green" are likely to be shortened to "green."

Figure 1 presents evidence that frequency distributions of color words vary greatly in their shapes, a fact corroborated by the results of a comparative study of English and Zuni (an American Indian language) color terminologies (5). With these findings at hand, it seems imperative that the role of these different shapes or *probability gradients*, as they will be called hereafter, be investigated experimentally.

Next it was determined how well an average English speaker can judge the probability of a given color to be called by a stipulated name. After all, the data presented in Fig. 1 were gathered from individual responses. Now we are asking whether a person who would call a certain color "greenish-yellow" is aware that that color is more likely to be called "yellowish-green" by the speech community at large; or whether a color that he calls "olive" in a given context might be called "green" in another. Conversely, if a person hears the word "yellowish-green," does he realize that this word might refer to a range of colors some of which have a greater probability of being referred to by the speaker than other colors within that range. Common sense tells us that we are capable of anticipating agreement or disagreement about color names. But common sense does not tell us whether individuals are very accurate in their estimate of these indeterminacies.

Thirteen Radcliffe College students were instructed to rate the likelihood of the 21 stimulus colors being called "brown," "green," "blue," "rose," or neither. In a

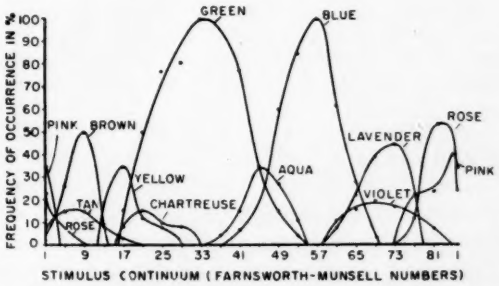

FIG. 1. Frequency Distribution of Color Names Elicited from 27 *Ss*.

The percentages of names for individual colors do not always add up to 100 because only the most frequently occurring names were plotted

pilot experiment it appeared that the *Ss* were confused when asked to express this likelihood in terms of percentages. After experimentation with several estimation methods it was found that a rating scale ranging from eight to zero could be handled adequately and with consistency. The following written instructions were used:

This is an investigation of language behavior. At present we are interested in finding out the actual usage of color terms. On the table you will find some color caps and a sample score sheet. Your task is to pick up one cap at a time. Write its identification number on the score sheet. Turn it over so that you can see the color. You are now asked to judge how applicable the color names on the sample score sheet (i.e., brown, green, blue, rose) are with respect to your color cap. Make your judgment in terms of a grade scale ranging from 0 to 8. Study the examples on the sample score sheet together with the respective color caps. Use grade 8 if the cap is an ideal example of one of the color words on the score sheet. E.g., cap #93 is graded "brown 8" because the grader thought it is the brownest brown possible. Everybody would call it brown. Use grade 1 if the name is *not ordinarily* used but if you think there is the barest possibility that the cap might be called by that name. E.g., cap #112 is graded "brown 1" because it is usually called beige but if a person can't think of that name he might exceptionally call it light brown. One might say that cap #112 belongs to the group of browns but that it is a very poor example of that group. Grade 4 is the midpoint of the scale. Cap #94 received grade 4 for both green *and* blue because in the grader's opinion both of these words were applicable to the same degree. He chose grade 4 because the color is a medium good representative of the groups of greens and blues. He thought people are as likely to call it green as blue. Note that more than one name may apply to a color. In that case judge the

applicability of both names. Your judgments for a given color need not add up to eight. If you leave a blank space behind a color name, you have made an implicit judgment: "zero or no applicability."

Twelve colors (not part of the Farnsworth-Munsell Test) were used for practice rating. During this "dry run" the *E* discussed with the *S* every one of her ratings so as to be sure that the same criteria for rating were used by all *S*s throughout the experiment, thus making it possible to compare the average of the estimated frequency of occurrence of color names (estimated "applicability of names") with the actual frequency of occurrence of those names. Correlating these two variables a coefficient $r = .91$, ($N = 23$), was obtained which meets the .01 significance criterion. Since all *S*s spoke English natively, we may assume that the highly significant correlation was due to the fact that speakers actually learn the probability of name-occurrence and that the rating method used is capable of reflecting this ability. (For the purpose of this particular correlation, equality of scale intervals had to be assumed. All subsequent statistics dispense with this assumption.)

Experimental procedure. How is it possible to change experimentally the frequency distributions that are characteristic of English color names? Suppose we show our collection of twenty-one colors eight consecutive times to an *S*. Every time that a perfect "green" comes up we shall say "green." The stimulus material of the experiment contained two colors which were always and unanimously called "green" by the Wellesley students and therefore we shall call each of these two colors "green" at all eight presentations. If a "blue-green" comes up, we shall sometimes call it "green" and sometimes "blue." For instance one color was called by 37% of the Wellesley students "blue," by 50% "green," and 13% "aqua." This color we might therefore call four times "green," three times "blue," and once "aqua." In this manner we can approximate the distribution obtained from the Wellesley data, or we may construct distributions that constitute distortions from the observed ones.

In the experiments testing the effect of varying the probability gradients, *S*'s task was essentially to watch *E* silently while he presented the entire color collection twelve times. Each time he presented a color he called it by a certain name. Some colors he called persistently by one and the same name, but other colors he called sometimes by one name and sometimes by another, in accordance with a pre-established distribution. The colors were presented one at a time, in twelve fixed random orders and the proportion of names given to each color was, wherever possible, the same within every sequence of four entire presentations. For instance, if a color was to be named 50% of the time "green" and 50% of the time "blue," then during the first four presentations it would have been called twice "green" and twice "blue." The same proportion would hold for the next four presentations and also for the final four presentations. But within these limitations the alternation of "green" and "blue" for this color was random.

The *S*s were not told specifically that their task was to discover the distributions of names. Instead, an effort was made to give them a mental set of a language-learning task. The procedure was this: first the *S* read the instructions quoted in the preceding section and then he was given practice in using the rating scale much the way the earlier group of Radcliffe students was trained. After this he was told that *E* was actually concerned with the investigation into the mechanisms of infant language learning. In order to reproduce a somewhat similar situation in the laboratory, *E* was now going to teach him a foreign language consisting of four words, all of which referred to colors. The *S*'s task was to learn the meaning of these words. The further course of the test was then explained, thus: *E* will present one color at a time and give that color one of the four "foreign" color names. He will do this for quite some time while *S* watches silently. After a while *S* will be given a score sheet arranged like the one he had practiced on except that now the four foreign words (hekachona, sossona, ashena, lhiana) will appear on the practice sheet.

At that time E will bring up every color once, without naming any and S will have to decide each time which name or names he wishes to mark and also how to rate the appropriateness or likelihood of occurrence of the given color names, using the rating scale studied before.

To give the task a more realistic appearance, the four foreign words were Zuni words and S was told that E had actually studied Zuni naming behavior and was reproducing it here in the laboratory. (It is well to bear in mind, however, that the latter of these assertions is a fiction. Actually, Ss merely learned straight or distorted replications of English naming behavior.) It was explained further that the colors which he was about to see had been taken to the Southwest, and that a group of thirty Indians had named these colors for E and that the Indians, just like English-speaking people, had not always agreed on what name to give each of the colors so that some colors are sometimes called this and sometimes that. It was pointed out once more that S had learned to use the rating scale in order to express the degree of agreement and that this was just another way of deciding "how good a green" a certain color is. The test did not begin until it was clearly understood that E was going to play the roles of some thirty Indians and that S was to be an Indian baby trying to learn the meaning and usage of four color terms.

Ninety-six Harvard and Radcliffe students were used for these experiments. They were randomly assigned to one of six experimental groups (16 subjects per group). Each experimental group learned a different type of probability gradient. Group 1 was a control. In this group subjects learned Zuni words distributed in exactly the same way as English distributes the words "brown," "green," "blue," and "pink" (girls are likely to prefer the word "rose"). The shape of the gradients is depicted in the solid curves of Fig. 2. Groups 2 and 3 had to learn probability gradients that were distortions of the actual English one. Group 2 had the steepest gradients with very narrow transitions between classes. That is to say, for this group colors were always given

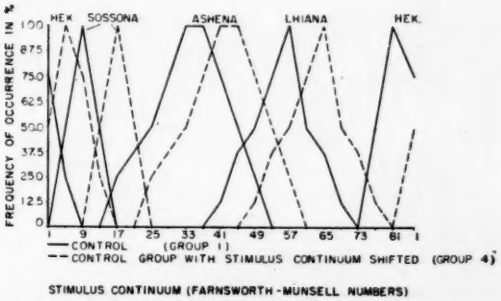

FIG. 2. Frequency Distribution of Names for the Two Control Groups (1 and 4).

The slopes of the curves are close approximations to the slopes of actual, English word distributions. Where percentages do not add up to 100, S was told that some of the Indian informants had used entirely different color words.

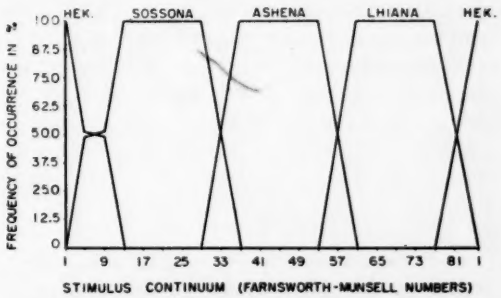

FIG. 3. Shape of Frequency Distributions of Names Learned by Ss in Groups 2 and 5 (increased determinacy).

Slopes are somewhat steeper and transition areas between categories are narrower than in the corresponding English words.

the same name; certain colors were always called "ashena" (green) or "lhiana" (blue) so that there was no overlap of categories. Here the spectrum was cut up into all-or-none categories. We might say there was a minimum of random behavior because the probability gradient was distorted so as to *increase* determinacy when compared with Group 1. The cuts separating one category from another occurred at that place in the spectrum where in English one distribution intersects another. The gradients of this group are shown in Fig. 3. Group 3 is the very antithesis of Group 2. These Ss learn the shallowest possible gradients with very broad transition areas. Between the most typical example of one category and the most typical example of a neighboring one, there are many colors that are either called A or B. There is considerable randomness,

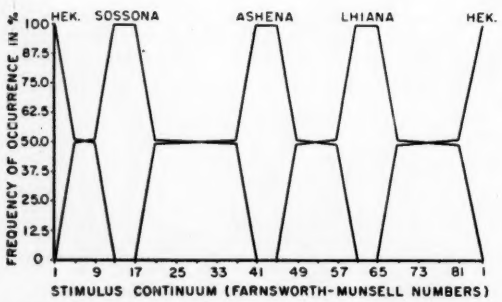

FIG. 4. Shape of Frequency Distributions of Names Learned by *Ss* in Groups 3 and 6 (decreased determinacy).

Slopes are less steep and transition areas between categories are wider than in the corresponding English words.

as may be gathered from Fig. 4, because the probability gradients were distorted so as to *decrease* determinacy when compared with the control group. The peaks of each of the categories coincide exactly with a peak in English and the transition area between classes is also in approximately the same location of the spectrum as the English transition areas.

Note that both Groups 2 and 3 deal with distorted versions of the distribution learned by Group 1, but that these distortions are essentially of the same kind: the slope of the original distribution is once made as steep and once as gradual as possible. Theoretically, we might have introduced any number of intermediary steps between these two extremes and it may thus be seen that we are dealing here with a continuous variable. But this variable is only one parameter of possible distortions of the original distribution curve. Another parameter is the "horizontal" dislocation of the curve on the stimulus continuum. Such a dislocation might be accomplished in the presence or absence of any changes in the slope of the distribution, as may be gathered from the following description of the tasks which confronted Groups 4 through 6.

The relationship of Group 4's task to that of the first group's is best illustrated in Fig. 2. It will be seen that the shape of the probability gradients is identical for the two groups but that their respective location in the spectrum is different. The *Ss* in Group 4 had to learn that colors for which there is a certain terminological randomness in English are now the most typical instances of a name class in the language to be learned. In other words, what these *Ss* knew to be in English a "yellow"-*or*-"green," was now a perfect example of "sossona." Conversely, a color which to English speakers is known to be a clear-cut case of "green," never to be called by any other name, would for this group be between "sossona" and "ashena," sometimes to be called by one and sometimes by the other name. Comparing the frequency distributions of Group 1, i.e., nonsense words distributed as ordinary English color words, with those of Group 4, it appears that the former distributions have peaks where the latter have valleys or class borderlines, and vice versa.

Group 5 learned gradients of practically the same shape as those in Group 2 but the borderlines between their classes did not coincide with English borderlines; instead, for this group the borderlines occurred where there are peaks in their native English.

Group 6 was presented gradients of the same shape as those in Group 3 but their peaks occurred where English has valleys and their interclass transition area was located in the spectrum where English has peaks.

Table 1 summarizes the relationship between the six groups of subjects.

Note that the response consisted of estimates of probability of occurrence of color names. Every *S* filled in three score sheets:

TABLE 1

RELATION BETWEEN SIX EXPERIMENTAL GROUPS OF SUBJECTS AND THE RESPECTIVE DISTORTIONS OF THE ENGLISH WORD FREQUENCY DISTRIBUTIONS

Horizontal Shift Of Curves	Changes in Slope of Curves		
	None when compared with English	Steepest slope possible	Most gradual slope possible
None: spectral location of curves as in English	Group 1	Group 2	Group 3
Maximal displacement of curves with respect to English	Group 4	Group 5	Group 6

one after the first four entire presentations, one after the next four, and one after the next four, i.e., the 12th entire presentation. These score sheets constitute the first, second, and third trial.

Since *S* had no feedback, reinforcement of the estimation response was practically nil. Even though *S* may have anticipated color names during *E*'s presentation and thus may have had an opportunity to compare his anticipation with what *E* actually called that color at that time, it must have been clear to *S* that a "wrong" anticipation may still have been consonant with a hypothesis such as "this color has a 50/50 chance of being called *A*."

RESULTS

In order to see what the *S* has actually done or how well he has learned the classification scheme, the raw scores must be plotted on graphs such as those shown in Fig. 5. The broken-line curve represents the actual distribution; the solid line has been constructed from the *S*'s estimates. For greater clarity his estimates were plotted individually for every color category. This also emphasizes the fact that the *S*'s estimates for one color category are independent of his estimates for another category because he was also told that the total estimates for a single color need not add up to eight.

Measure

At first sight it may appear as though the most interesting measure were the discrepancies between the actual and the estimated distributions. From a theoretical point of view this would indeed be an interesting study, but to measure the data in these terms would have required a number of assumptions which the writer is not prepared to make at this time. Nor would the measure have been directly relevant to the practical problem under investigation, namely: "If the *S* were actually asked to communicate in the pseudo-language he has learned, how well could he be expected to do, judging by his performance on the test?" Two criteria were used to evaluate this performance. The first criterion is formulated in the question, "Has the *S* learned to locate the borderlines (henceforth referred to as *cuts*) that separate

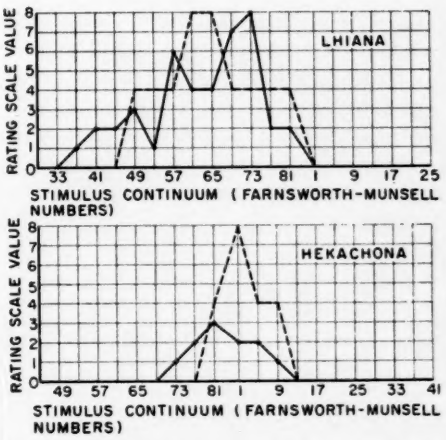

FIG. 5. Illustration of How Raw Data Are Plotted for Evaluation and Scoring.

Broken curves represent the distributions actually presented to *S*. Points on solid line correspond to individual entries on *S*'s score sheet.

the four color categories from one another?" And the second criterion is expressed in the question, "Was the *S* consistent in his response?" Note that the first criterion allows us to ask whether a given response (i.e., an entry behind a color name) is correct or incorrect. If the *S* calls a color "green" that has not once been called "green," he has given a wrong response. Since the colors might be ordered linearly and were equidistant, we may also ask how grave the *S*'s mistake was; if he calls a color "green" that is four steps removed from the last color ever to be called "green," he has made twice as bad a mistake as if he had called a color "green" that is only two steps removed from the actual last "green." It is well to remember here that the distance between each adjacent color is considerable (somewhat greater than six j.n.d.'s), so that a mistake of four colors removed is a mistake comparable to calling a color "pink" that is usually called "yellowish green"; making a mistake of two colors removed is like calling a color "pink" that is usually called "light brown" or "tan."

The definition of the consistency criterion is not as simple as that of the criterion for the location in the spectrum (cuts). Inspection of all the frequency distributions used (Figs. 2, 3, 4) reveals that they have this in common: no distribution has more than *one*

maximum, i.e., there is only one peak in each curve. The curves rise (or remain at a constant level and then rise again) until a peak is reached; once a curve begins to fall, it never rises again. If a person in Group 1 (cf. Fig. 2) calls color #29 "green" and assigns it a probability of, say, .8 and if he calls color #37 "green" with a probability 1.00 (i.e., he judges color #37 to be a perfect example of the class "green") then that person must also call color #33 "green"; if he is consistent, he will assign it a probability between .8 and 1.00 but not lower. It would be an inconsistency to call color #33 a good example of "blue" (i.e., assign it a probability zero for "green") or even to call it a "poor green," say probability .25, after color #29 (which actually contains more yellow than either #33 or #37) and color #37 had been judged to be good representatives of the class "green." When inconsistent responses are plotted (see Fig. 5) we obtain a *jagged* distribution curve. Each *jag* corresponds to one inconsistency. The estimated frequency distribution of "lhiana" in Fig. 5 has two jags—two inconsistencies—but the estimated distribution of "hekachona" has no inconsistencies. Note that the measure of consistency (hereafter called the jag-criterion) is not a measure of the goodness of the fit of the estimated distribution to the actual distribution. It does not measure the accuracy of the probability estimate as such but the *S*'s ability to make judgments that are consistent with one another. Thus we do not have to make the assumption of "equality of intervals of the rating scale." By considering *jags* instead of the individual probability values, we are actually focusing on the *rank order* of judgments, that is, we are transforming an interval scale into an ordinal scale which, as pointed out by Stevens, is an "order-preserving transformation that will leave the scale form invariant" (8). The following rules were observed in scoring *S*s' performance.

Cuts: Count lateral deviation between estimated and actual cut; make no distinctions between deviations to the left or to the right of the cut; disregard a lateral deviation of only one color step, allowing for small vacillations due to memory or discriminability between adjacent colors; disregard all lateral deviations where the estimated probability is not larger than .12 (i.e., 1 on rating scale), allowing for imperfections in the scaling method.

Jags: Count number of jags; a jag is defined as a twofold reversal of the trend of a distribution curve; disregard a jag if it is less than two cells deep on either side (to allow for imperfection i.. the scaling method); a jag that reaches the zero line counts double, because it is not only an inconsistency in terms of probability estimates but it also represents intermixing of color categories.

A *jag* is not *a response* in the proper sense of the word. It is actually the *relationship* between not fewer than three responses; it is a response *pattern*. When we deal with language it seems particularly important to deal with response patterns instead of dealing exclusively with individual responses, because language, in literally *all* its aspects, is a pattern-phenomenon. Words and sound sequences are intelligible, not because the individual sounds (phones) are physically invariable from instant to instant, but because the *relationship* between the phones is invariable (cf. a word once uttered by a male and once by a female voice where each "male phone" is different from the corresponding "female phone"). The same thing is true in the realm of reference (*vulgo* meaning). If a person has called a pea-soup color "green," we expect him *a fortiori* to call the color of grass "green." But if he had called pea soup "ecru," then we would not be surprised if he called the color of grass "deep, greenish chartreuse."

The scores obtained by the six experimental groups are tabulated in Table 2. The high correlation between jag scores and cut scores ($r_r = .87$) seems to indicate that we are measuring a common underlying factor which we might call the *S*'s adjustment to the learning situation. In order to obtain a single set of scores expressing *S*'s general performance on the test we may, in this case, add an individual's jag scores to his cut scores.[3] A graphical presentation of the scores thus combined is offered in Fig. 6. Table 2 shows the standard deviations of the combined scores so that the nature of the distribution of these scores may be determined. It will readily be seen that we have here a Poisson distribution ($\sigma = C \cdot m^{0.55}$

[3] I am indebted to Robert R. Bush for advice on this matter.

TABLE 2

SUMS OF SCORES FOR SIX EXPERIMENTAL SITUATIONS

(N = 16 in each group)

Trials and Scores	Location of Categories as in Engish			Location of Categories Displaced with Respect to English		
	Group 1: Control	Group 2: incr. determinacy†‡	Group 3: decr. determinacy‡	Group 4: Control	Group 5: incr. determinacy†‡	Group 6: decr. determinacy
First Trial						
jag scores*	16	33	80	37	80	106
cut scores*	44	41	89	38	85	173
SD of jag+cut scores	3.24	4.58	6.95	3.13	6.63	8.49
Second Trial						
jag scores	21	25	61	33	57	122
cut scores	26	14	49	27	77	116
SD of jag+cut scores	2.81	2.57	6.32	5.13	7.35	9.34
Third Trial						
jag scores	20	13	61	24	27	91
cut scores	29	13	48	32	38	125
SD of jag+cut scores	3.51	2.21	6.67	2.9	3.26	10.25

Note.—The lower the score, the better the performance.

* Reject H_0 with $p < .05$ that scores in this row are distributed equally over 6 groups.

† Reject H_0 with $p < .05$ that jag scores in this column are distributed equally over 3 trials.

‡ Reject H_0 with $p < .05$ that cut scores in this column are distributed equally over 3 trials.

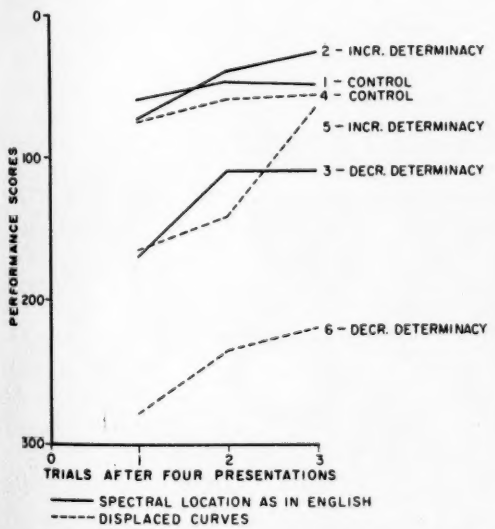

FIG. 6. Rate of Improvement in General Performance.

Points were obtained by adding jag scores to cut scores. The lower the score the better the performance.

TABLE 3

SUMMARY OF ANALYSIS OF VARIANCE OF COMBINED, TRANSFORMED SCORES

Source	*df*	*F*
(*a*) Displacing categories with respect to English	1	45.77**
(*b*) Varying determinacy	2	53.45**
(*c*) Trials	2	11.04**
Interaction (*a*) × (*b*)	2	6.73*
Interaction (*a*) × (*c*)	2	.39
Interaction (*b*) × (*c*)	4	1.18
Triple interaction	4	.19
Error variance	270	3.89†

**Significant at $p = .001$.

*Significant at $p = .01$.

†Mean square.

where C is a constant) so that a variance-stabilizing transformation is indicated in order to perform an analysis of variance. Freeman and Tukey (3) suggest the square-root transformation $\sqrt{x} + \sqrt{x+1}$ instead of just $\sqrt{x + .5}$ and Mosteller and Bush (6) provide a convenient table, values from 0 to 50, for the use of this transformation. A summary of a three-way analysis of variance of the transformed, combined scores is given in Table 3.

DISCUSSION

The tabulated results indicate that, in terms of general performance, there is a statistically significant improvement from first to third trial for the entire test population (i.e., when the data from all six groups are pooled). But if we study the rate of improvement of the six groups individually (see Fig. 6), it becomes obvious that the rate is dependent upon the *slope* of the probability gradient. However, shifting the stimulus continuum has no significant effect on that rate. In the control groups (1 and 4) where the slope of the probability gradient is the "customary English" one, there is least improvement from trial to trial. This is not surprising because the scores at the first trial are already so low that little improvement is possible.

A distortion of the customary English slope either in the direction of increased determinacy, or decreased determinacy,

results in an initially more difficult task. This is interesting because we would expect from a common-sense point of view, that the absence of random naming behavior would decrease ambiguity and hence turn the situation less confusing. But this is not what happens. The *S* is evidently conditioned, by virtue of speaking English, to *expect* a certain degree of indeterminacy in certain parts of the spectrum. If, instead, he finds order in these parts, he is at first confused. However, if the stimulus continuum has not been shifted, he quickly orients himself and the absence of randomness soon becomes a facilitating factor. He learns so rapidly that at the third trial his performance is significantly better than the performance of the subjects in the control group.

Changing the slope of the probability gradient so as to *decrease* the determinacy makes the task considerably harder than an increase in determinacy. In fact, it is so hard at the beginning and the scores are so high at first, that there is the same rate of improvement between the first and second trial as observed for the group with increased determinacy. But for the increased determinacy group the rate of improvement is further accelerated after the second trial, whereas the decreased determinacy group ceases to learn after the second trial; the learning curve reaches an asymptote here. Decreasing the determinacy seems to be a continuing handicap in the *S*'s comprehension of the classification system that he is expected to learn.

Inspection of the raw scores indicates, and the results of the analysis of variance confirm, that shifting the stimulus continuum has an effect on performance of *S*s in all six groups. (Note that we are not speaking here of the *rate* of improvement, as above, but of absolute performance.) But Fig. 6 makes it quite clear that such a shift is of relatively small importance as long as the slope of the probability gradient is not disturbed. If the *S* finds a degree of randomness such as he is accustomed to from English, he has little difficulty in learning the new, shifted classification system. The fact that he finds a peak where he is used to having a trough does not confuse him. Even at the first trial he finds the cuts that separate one color category from another with the same ease as *S*s in the control group who learn completely undisturbed gradients. From Fig. 6 we gather that shifting the stimulus continuum, leaving the slopes of the probability gradients intact, creates a slightly unfamiliar situation but does not present any marked confusion.

If the slopes of the probability gradients are distorted in addition to the shift of the stimulus continuum, we have bedlam. This is even true of the group with increased determinacy. If the stimulus continuum is shifted, yet the randomness diminished (the slope of standard English being the baseline of the comparison), the task is still more confusing than that faced by the decreased determinacy group whose stimulus continuum had not been shifted. Most difficult of all six situations is the one in which determinacy is decreased and the stimulus continuum shifted. Even at the third trial the achievement of subjects in this group is lower than under any other condition in the experiment.

There is good evidence that the difficulties arising from shifting the stimulus continuum are not merely due to the fact that *S*s have never actually recognized the shift and are simply classifying the colors as they are used to doing in English. First, if we compare the mistakes made on the cut score by the control group with those made on the same score by the control group with shifted continuum, it is seen that the variation between the two is statistically not significant. (In order to make the test we transform the cut scores by the Freeman-Tukey transformation, compute the means of these scores and then make an ordinary t test assuming equality of variance for the two samples.) The meaning of this result is that *S*s under the shifted condition have no greater difficulties in finding the cuts than the control *S*s. If the *S*s under the shifted condition had simply adhered to their English custom, they would have made a systematic mistake toward the English location of the cut. A second piece of evidence is the comparison of scores made by the same two groups (group 1 and group 4) on the jag criterion. The numbers tabulated in Table 2 are counts

of jags (irrespective of the magnitude of jags). Under the null hypothesis we assume that both groups have an equal number of jags at each trial, i.e., that they are equally consistent or inconsistent as the case may be. Since we are counting things of unspecified quality the appropriate test of significance is the χ^2 test, where the expected value is the difference between the two observations divided by 2 and we enter the χ^2 table with one degree of freedom. The test indicates that the difference between jag scores for the two groups is significant ($p < .02$) in the first trial. The conclusion we must draw is that the *S*s, under the shifted condition, have little trouble in finding the respective color classes on the spectrum but that they are not certain of themselves and are distinctly more inconsistent in their answers than the control group.

How can we explain the very poor performance of *S*s who learn gradients that are distorted so as to decrease the determinacy? Is this simply a consequence of having systematically confused the *S*? This is hardly a sufficient reason. In the distributions of the two control groups (1 and 4) there also occur colors to which 50% of the time name *A* was applied and 50% of the time name *B*. These "50/50 colors" were compared with the same colors in the decreased determinacy group (comparing the two non-shifted groups with one another and the two shifted ones) to see whether estimation of a .5 probability is more difficult under any of these conditions. There was no significant difference in the estimation of .5 probabilities between any of the groups compared. The control group did as poorly (or well, depending on how we wish to evaluate the case) as the experimental groups. From this we must infer that the variable *performance* is independent of the variable *estimation of probability* because it is possible to obtain a high performance score even if there is a constant "mis-estimation" of the individual probabilities. In other words, the increased number of 50/50 colors cannot account by itself for the poorer performance of the decreased determinacy group.

Note that discrimination cannot account for the experimental results; a little reflection about the design of the experiment will make this obvious and an elaboration unnecessary.

In short, we have the following findings: (*a*) The *entire* configuration and probability structure of a classification system is the main factor regulating *performance* on a classification task. The probabilities assigned to *individual* colors play no significant role in the over-all performance. (*b*) Shifting the stimulus continuum with respect to the classification system affects performance in cases where the stimulus material has been previously conceptualized. (*c*) The slope of the gradient to be learned plays an extremely important role in the performance of a classification task. Particularly, it affects the speed of learning, a steep gradient being associated with fast learning; a shallow gradient with slow learning. (*d*) If stimulus material has already been conceptualized by adult *S*s, certain probability gradients have been learned and it becomes difficult for the *S* to rid himself of the influence of this earlier learning.

We have here a clear case of negative transfer of learning. The initial difficulty experienced by the increased determinacy group reflects the importance of the transfer, whereas the rapid learning rate indicates that a category with steep probability gradients is actually easier to learn than one with a less steep gradient. It is likely that ideally naive *S*s, such as children, learning their first language, would obtain best scores in the case of maximal determinacy, even on their first trial.

SUMMARY

Six groups of *S*s were taught nonsense languages. The languages consisted of four color words and the *S*'s task was to discover the "meaning" of the words by silently observing the speech behavior of an entire language community. The words of the first language were distributed over a stimulus continuum exactly as the English words "brown," "green," "blue," and "pink." This constituted a control group. The other five languages had words whose frequency distributions represented systematic distortions from the previously established English

distributions. There were two types of distortions: distortions of the slope (increasing or decreasing the determinacy of color names) and distortions of the location on the physical continuum (making colors, which in English have no specific name, a perfect example of a color category in the nonsense language). The distortions were the independent variable; the *S*'s achievement in learning to use the color words was the dependent variable. The main results were: If the reference relationships in a nonsense language are identical with those in the *S*'s native tongue, the learning task is relatively easy; but distortions in these relationships make the task harder. This is even true if the determinacy in naming behavior is experimentally increased beyond a "customary" degree. However, in this case learning is very rapid and after a short while *S*s do better than if they had to learn a language with reference conditions and degrees of determinacy identical with those in their native language. An experimental decrease in determinacy of naming behavior results in a more difficult learning task. A shift of the category location on a stimulus continuum is far less disturbing for second language learning than any change in the probabilistic aspect of reference. There is good evidence that the shape of word frequency distributions over stimulus continua regulates the ease with which a person learns to use a word "correctly." A distribution with steep slopes seems to be associated with rapid learning, whereas gradually rising distributions seem to retard learning.

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Every known fact in natural science was divined by the presentiment of somebody, before it was actually verified.

Ralph Waldo Emerson in *Nature*

SITUATION DYNAMICS AND THE THEORY OF BEHAVIOR SYSTEMS[1]

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Is it possible to identify and to quantify variables in terms of which the behavior of an individual as a member of a group, such as a child in his family, can be described? Here a set of such variables is proposed together with ways of measuring them. The variables have to do with degrees of activity, of emotional content, and of external pressures involved in various forms of behavior. Further, a set of functional relations is postulated among these variables. From these further quantitative relations are derived, which are put to experimental test. In general, some interesting corroborations of the model are obtained.

In recent years, the basic unity of the behavioral sciences has become increasingly recognized. Stimulated particularly by the study of small groups, both psychologists and sociologists have become aware that the difference between psychology on the one hand and sociology and social anthropology on the other does not lie in the type of phenomena studied, but in the frame of reference employed in the analysis of the same basic data. In studying a group, we use the data on the activities, perceptions, attitudes, and power relations of members in order to define the properties of that group. In studying an individual we use the same data in order to determine his personality characteristics. Since in the study of behavior at either level we are concerned with the identical system of events, it follows that a theory of behavior must permit the representation of both psychological and social structures by means of the same set of basic concepts, and furthermore permit the derivation of both psychological and sociological laws.

The theory of behavior systems in its present form has its source in Gestalt and field theoretical concepts. It departs from the approach formalized by Lewin (15) and Heider (7) by eliminating the distinction made between the person on the one hand and his behavior on the other. The person is defined in terms of his behavior. This leads to the representation of a behavior process as a change in state of a behavior system rather than the locomotion of a person through his behavior space. The basic components of a behavior system are taken to be the activities engaged in by individuals since all other components and characteristics of behavior are ultimately defined in terms of or with reference to activities. Attitude, communication, and power structures are consequently regarded as secondary phenomena which can be meaningfully studied only with reference to a functioning activity system.

In an earlier attempt at formulating a system approach (8, 9), the family looked at as a 2-person group was analyzed in terms of its activity and power relations together with its associated tension properties, taking individual members and the family group as a whole as alternative frames of reference in the analysis of data. The conceptual framework was later extended so as to include the cognitive representation of the activity system and its associated properties (10). The individual at this stage was looked at as an open system with respect to the group of which he is a member and the situation existing for him as a group member

[1] The empirical testing of the theoretical formulation was carried out while the author participated in an interdisciplinary study of "Technological Change and Social Adjustment" at the Behavioral Research Council of the University of Pennsylvania, 1952–53.

was analyzed in terms of the activities available to him and the perceived internal and external pressures operating within the given situation.

The present paper is concerned with a reformulation of the theory especially with respect to energy and equilibrium concepts. We shall discuss initially the basic system concepts together with the concepts required for measuring a situation, derive and test a set of functional relationships between situational variables, and consider the relation between alternative frames of reference in the study of behavior systems.[2]

THE METHOD OF THEORY CONSTRUCTION

Choice of basic concepts

In the development of a theory the first step is the choice of a set of basic concepts or dimensions, together with a set of operations which may be performed on these concepts. The requirement is made that all concepts needed for the representation of behavior phenomena can be defined in terms of the basic ones.

A set of basic dimensions is not unique. In the theory of mechanics, mass, length, and time are generally taken as the basic dimensions and all further concepts needed to represent mechanical phenomena, such as force, momentum, acceleration, etc., are defined in terms of these dimensions. However, we can equally well choose force or energy, instead of mass, as one of our dimensions. The distinctions between basic and derived concepts are thus relative.

The discovery of a set of suitable dimensions for behavior theory is not primarily a problem for empirical experimentation but for conceptual experimentation. The aim is to find a set of concepts in terms of which all concepts required to represent the structure and properties of behavior systems can be defined. If a given set of concepts does not accomplish this purpose, then a new set has to be tried out.

[2] Among present-day convergent approaches toward a system theory we may mention Bertalanffy and Ashby in biology, Krech and Newcomb in psychology, Ruesch in psychiatry, Parsons in sociology, Henry and Homans in anthropology, Trist in socio-technical theory, and Samuelson and Tustin in economics.

The choice of dimensions is closely related to the size of units studied. This is a problem of research strategy. While it would not be wrong to represent a physical object in terms of the position, velocity, and interrelation between the molecules of which it is composed, it would be impractical, if not impossible. The most efficient size of units will be the most macroscopic, which is still sufficient to represent the object studied. Thus for most purposes in the theory of mechanics, an object may be represented as a point region with a coordinated vector. The more microscopic our unit elements become relative to the object studied, the more information is required to represent that object, the amount of information being measured in terms of the number of independent parameters required to represent a given assembly or system. The choice of the most macroscopic unit possible thus means essentially that the most efficient size of unit elements is one that gives a complete representation of the system studied with the minimum number of information elements.

Behavior theory must be based on behavior concepts. We may study individuals as behavior systems in a behavioral environment or as biophysical systems in a physical environment. These two approaches are distinct, differing both in subject matter and the type of concepts appropriate to each. It is not, of course, asserted that biophysical events have no influence on behavior. These, however, will not affect the laws of behavior systems just as human interference in the physical universe, while affecting the course of physical events, does not affect the laws of physics.

While in the construction of a behavior theory physicalist and physiological concepts must be excluded, analogies from other sciences may at times be useful. At least in their early stage the physical sciences freely used behavior concepts and analogies in the building of their models. Atoms were thought of as having male and female characteristics, unlike atoms attracting one another and like atoms repulsing one another. Objects were endowed with "energy" and regarded as capable of doing "work" under some conditions and losing energy and becoming less

capable of doing work under other conditions. What is important is that terms which had been taken over from behavior language, such as impulse, energy, attraction, force, work, etc., were ultimately coordinated by analogy to purely physical constructs. The derivation of all concepts employed in physics from a set of basic physical concepts made it possible to exclude the behavioral and anthropomorphic content of the initial models. One of the aims in attempting to derive all concepts in behavior theory from a set of basic behavior concepts is the exclusion of mechanomorphic and biophysical content from models of behavior systems. The integration of the biophysical and behavior sciences appears to be a problem for the future. The building of a bridge between them demands the existence of reasonably firm foundations on both sides.

Unit concepts and operational coordinates

Having decided on a choice of general system concepts, the next step is to coordinate both the basic and derived concepts to specific unit concepts. The unit studied may vary from a situation existing for an individual, the total behavior system of an individual, a small group, a society, or a group of societies. In each case a *different* set of unit concepts will be coordinated to the *same* set of system concepts. Thus if our unit of study is a situation existing for an individual, then stress which is a derived system concept is coordinated to the extent of opposition of internal and external forces operating within a given situation. If our unit of study is a small group, then stress is coordinated to the extent of opposition of internal and external forces operating within the group.

Finally, we need to relate each set of unit concepts to operational coordinates. That is, we have to decide how to coordinate the various observed phenomena to the various unit concepts and what methods of measurement to use. It should be noted that while in some cases the coordination of system to unit concepts and of the latter to operational coordinates is a straightforward matter, all such coordinations are in the nature of hypotheses which need to be tested in conjunction with the postulated and derived relationships between variables.

Since the various units correspond to different frames of reference in the study of behavior events we shall find a number of relations between concepts employed in the representation of different units. Thus in moving from a study of the situation existing for an individual as a member of a group to a study of the group as a whole, the external pressures deriving from the group which operate on the individual now become part of the internal forces operating within the group. At the same time, disagreements and overt conflict which the individual has with other members of the group correspond to internal strain with respect to the group, but to external adjustment processes with respect to the individual. One of the major aims of a system theory will be to discover those principles of behavior which remain invariant under translation of reference frames.

THE ELEMENTS OF BEHAVIOR SYSTEMS

A *system* is defined as a boundary-maintaining set of interdependent elements. In this sense a biological organism would also be regarded as a system. The difference between a biological and a behavior system lies both in the type of elements of which each is composed and their process structure. Three types of basic elements are regarded as sufficient to represent the behavior system of an individual or that of a group. These are (*a*) *activity elements*, which stand for activities such as washing the car, preparing a meal, or going to the pictures, (*b*) *cognitive elements*, which are defined as a representation or coding of activity elements and their related properties, and (*c*) an *energy* variable associated with each element contained in the system.

The term *domain* will be used to refer to systems consisting of homogeneous component elements. The set of activities which the individual engages in defines his *activity domain*. Similarly, his set of cognitive elements defines his *cognitive domain*. If the individual at any given moment engages in an activity A, his activity domain is said to be in state A. A change from activity A to activity B

is denoted by a transition in his activity domain from state A to state B. Activities may be included in or excluded from the activity domain thus changing the set of available activities which the individual may engage in. For the purpose of representation, activity elements are coordinated to areas located within or outside the activity domain.

It is postulated that the activity and cognitive domains are related by simple interdependence so that changes in the activity domain will result in the operation of pressures toward corresponding changes in the cognitive domain and vice versa. The relation between the two domains is thus circular, the whole functioning as a behavior control system. The degree of correspondence between the objective characteristics of the activity domain and its representation in the cognitive domain will be a function of the degree of interdependence of the two systems and the strength of processes toward representational distortion. The degree of interdependence fluctuates over time decreasing during periods of day-dreaming and sleep. With a reduction in the degree of interdependence, the cognitive domain, being less restrained by objective situational characteristics, is in a position to move toward equilibrium states determined by its own structural characteristics. In this sense, periods of relaxation provide the opportunity for the operation of cognitive processes which may be regarded as wish-fulfilling. The level of interdependence between the activity and cognitive domain during waking periods defines the degree of reality contact of the individual.

The activity and cognitive domains jointly define the *behavior system* of the individual. A *situation* existing for the individual, such as the child's situation in the family or at school, will then be represented by a region contained within his total behavior system.

Vitancy is defined as the cognitive representation of the amount of energy associated with a given activity area or region. Vitancy corresponds to the experience of involvement, emotional attachment, or love. Attachment or love may be with respect to activities related to other persons or objects, to processes and part regions within the behavior system or to the relation of the system to its environment, such as the possession of power and status, or experiential states and relations to the environment which are less easily verbalized, involving the total self rather than part functions. Attachments, whether toward others, aspects of the self, power, possessions or achievement, are dynamically equivalent. One may be substituted to a greater or lesser extent for the other by withdrawal of energy from one region and allocation to another. Attachment or love constitutes the primary process of behavior systems. It is a necessary condition for behavior to occur and provides the basis for the derivation of other behavior processes which are found to operate. Put in a noncognitive form, it is stated that energy allocation is a necessary condition for a behavior system to come into operation. A behavior system is said to be in a state of internal equilibrium if the amount of energy associated with the system is equal to the level of system functioning. A state of disequilibrium or *strain* is said to exist if the level of system functioning either falls below or rises above the amount of energy associated with the system. The degree of strain (T) due to the internal characteristics of the system is determined both by the amount of energy associated with the system and the extent of deviation of the level of system functioning (U) from the amount of energy (E) so that assuming the simplest relationship

$$T = \alpha E(E - U)^2. \qquad \text{[Definition 1]}$$

The family of curves which corresponds to this type of function is shown in Fig. 1. The definition implies that the effect of a given deviation from the internal equilibrium state on the strain level will increase with the amount of energy associated with the system. At least in a closed system strain will be zero either if the amount of energy associated with the system becomes zero or if balance is established between the level of energy and the level of system functioning.[3]

[3] For the purpose of a more general definition, strain may be defined in terms of the sum of deviations of a given level of functioning from a set of optimum levels weighted by the respective energies involved.

We shall refer to a *low level equilibrium* if a small amount of energy is balanced by a low level of system functioning. A *high level equilibrium* is said to exist if a large amount of energy is balanced by a high level of system functioning. It is postulated that *a behavior system will operate so as to maximize its level of equilibrium*. This is equivalent to stating that the system will operate so as to increase its level of functioning to a maximum and at the same time reduce its level of strain to a minimum.

The level of system functioning is defined in terms of three dimensions: (*a*) the size of the system in terms of the number of component elements, (*b*) the intensity of element states, and (*c*) the level of integration of elements. In terms of this definition Equation 1 shows that strain reduction may proceed in three distinct ways:

1. If the amount of energy is larger than the existing level of system functioning, strain reduction is achieved by increased growth and differentiation, increased intensity of functioning, and increased level of integration.

2. If the amount of energy is lower than the existing level of system functioning, strain reduction is achieved by contraction in size, dedifferentiation, decreased intensity of functioning, and reduction in the level of integration.

3. If no decrease in disequilibrium is possible by means of restructuring, strain reduction can proceed by a reduction in the amount of energy associated with the system.

A reduction in the energy level assuming no concurrent change in the level of system functioning leads to an initial rise in strain before strain decline occurs, so that under these conditions a strain barrier against energy withdrawal exists. This implies that energy allocations and thus also the attachments and experienced vitancy of the various components of the behavior system will tend to be relatively stable. Similarly, if the system has reached a steady state, reduction in the level of system functioning assuming no concurrent reduction in energy level results in an increase of strain, the rate of strain increase being a function of the amount of energy associated with the system. It follows

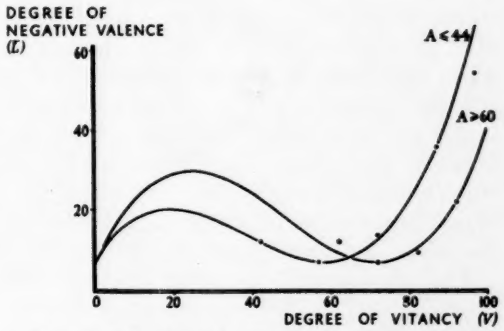

FIG. 1. Relationship Between Negative Valence and Vitancy of the Situation with Size of Activity Domain Constant Within the Range 44− and 60+ (Mean A = 39.7 and 65.7 respectively).

The theoretical relationship is given by Eq. 1a

then that once a given behavior structure is in equilibrium with respect to the available energy, resistance toward structural change in the form of a strain barrier will operate, the strength of the strain barrier increasing with the amount of energy associated with the system.

If the system were to be completely isolated and undifferentiated, then it would eventually reach a state of minimum strain after which no further change would occur. This is not found to be the case. Instead, a behavior system has to be looked at as part of a hierarchy of open systems.[4] Each activity area has as its environment the behavior system of the individual of which it forms a part. The latter again is an open system with respect to the social structure within which it is located.

Insofar as changes within a given component part of the system in the direction of an equilibrium state for that component may lead to a state of disequilibrium for the system as a whole, or vice versa, the system will be subject to pressures toward a state of quasi-stationary *strain balance*. Putting this in a slightly different form, we may say that, due to pressures toward system equilibrium, not all component parts can achieve a state of minimum strain which they would be able to achieve in isolation from one another, nor can the system as a

[4] Strictly speaking, we should refer to a partial hierarchy in the case of behavior systems, since the latter may in some cases also be found in a relation of overlap with one another.

whole achieve a state of minimum strain due to opposing forces of its component parts. Quite generally, then, every system is subject to some degree of constraint arising out of its interdependence with other systems. The extent to which external pressures operate in opposition to internal pressures toward an equilibrium state defines the degree of *stress* operating on the system.

The phenomenon of strain balance may be illustrated by the relation of individuals to the group within which they function. From the point of view of group functioning, overt strain is represented by conflicts and disagreements between members. Within the groups as a whole, pressures will therefore be operating to decrease disagreements. From the point of view of group members, however, disagreements represent interpersonal adjustment processes with the aim of decreasing personal strains. Group pressures operating so as to reduce the expression of disagreements will, therefore, lead to an increase in personal strains. On the other hand, an increase in interpersonal conflict beyond a certain point will lead to the disruption of the group as a boundary-maintaining system. The ultimate state arrived at if the group is to survive is, therefore, one of balance between pressures toward reduction of strain within the group as a whole and pressures toward reduction of personal strains by individual members.

The normal steady state of the system is one in which the amount of energy associated with it is higher than the level of system functioning. It is only under these conditions of available excess energy that the system is capable of growth, adaptation, and maintenance in its environment. Since the existence of excess energy implies the existence of a state of strain it follows that the capacity of the behavior system for development and adjustment is dependent on its ability to maintain states of disequilibrium and strain without internal disruption.

THE ELEMENTS OF A SITUATION

The derivation of situational variables

In order to determine the structure of a situation at a first approximation, we need to determine the forces which operate on activity areas toward inclusion or exclusion from the situational activity domain. From the basic structure of behavior systems outlined we can derive three distinct types of forces: (*a*) *Valence Forces*, defined as cognitive representations of strain states associated with individual activity areas; (*b*) *Internal Pressures*, defined in terms of the perceived strain effects on the individual's behavior system as a whole resulting from the inclusion or exclusion of activity areas, and (*c*) *External Pressures*, resulting from the interdependence of the individual's behavior system with that of other individuals.

Assuming for the moment that no distortion operates in the process of cognitive representation, then the direction of perceived internal forces can be directly derived from the characteristics of strain states in the activity domain. Let us consider the possible states of the system, keeping in mind that the level of system functioning with respect to a given activity refers at this stage simply to the extent of participation in the activity.

If the activity has no energy associated with it, then a change in the level of participation can produce no intrinsic strain. The activity in this instance will be associated with a *neutral valence*. Suppose that the activity is associated with energy and is thus perceived to be of intrinsic importance then to the extent that participation in the activity is associated with strain, pressures will operate toward ejection of the activity from the person's activity domain, and the activity is said to possess a *negative valence*. Similarly, to the extent that non-participation is associated with high strain, pressure will operate toward the inclusion of the activity in the person's activity domain and the activity is said to possess a *positive valence*. The relationship between the vitancy of an activity and its capacity to produce either positive or negative affect will at a later stage be used to obtain an indirect measure of vitancy.[5]

[5] Valence should be distinguished from affect. If a person is not in a position to engage in an activity which he wants to do then the activity has a positive valence but the situation with respect to the activity has negative affect. Affect (F) is a

The various activities which the person engages in are not independent of one another but form part of a system. The pressures that operate either toward participation or non-participation will, therefore, not be determined by the intrinsic characteristics of the activity alone but also its effects on the individual's activity domain as a whole. The direction of *internal pressures* operating on an activity is derived from the effect of participation or non-participation on the strain level within the activity domain as a whole. If inclusion of an activity in the activity domain leads to an increase in the strain level within the domain the internal pressures will operate toward ejection of the activity. Similarly if inclusion of the activity results in strain decrease within the activity domain, internal pressures will operate toward inclusion of the activity.

Two components of internal pressures have to be distinguished. There are first of all *ego pressures* resulting from the internalization within the system of previously external pressures, a process discussed in (10). Second, there are the *value forces* defined in terms of the perceived or expected effects of change in the location of a given activity element on the structure and location of other activity regions, and on the relation of the system to its environment. Valence and value are easily distinguished. Valence corresponds to the satisfaction or enjoyment derived from engaging in an activity. Its value is given by the benefits or losses to which it is expected to lead. Thus, the painting of a picture may provide intrinsic satisfaction for the painter and also extrinsic benefits such as the gain of money, status, and reputation. Valence and value forces may, however, also be opposed in direction if, for instance, the individual carries out work which he finds unpleasant but which contributes to the satisfaction of other needs. It should be noted that valence and value are relative terms where part functions of the system are concerned. The gain of money as a consequence of an activity may, in turn, have a positive valence by providing intrinsic satisfaction and also a positive value in terms of the uses to which it is put. With respect to ultimate states of the system as a whole, value and valence coincide, since at this level we are concerned with absolute states which have no end beyond themselves.

Coming to the level of operational definitions of system forces for the case of individual behavior, valences are coordinated with feelings of like or dislike for each activity, ego pressures are coordinated to feelings that one should or should not engage in the activity, and external pressures correspond to perceived demands made on one by other individuals or by the group.

The method of measurement

In order to test functional relationships between situational variables we need to employ a technique which enables us to obtain measures with arithmetical properties. The method used is based on behavior sampling and is discussed in greater detail in another article (9). We proceed by choosing a representative sample of situational activities and assume only that the individual can judge whether or not each activity does or does not possess a given characteristic. The extent to which the situation as a whole possesses a given characteristic is then measured simply by the proportion of activities which are judged to possess that characteristic. Thus the degree of positive valence of the situation is measured by the proportion of sampled activities which the person feels he likes doing or would like to do.

The following is the form in which the characteristics of each activity are measured for the child's perception of the situation existing for him as a member of his family.

1. Location of the Activity Area: whether the child engages in the activity always, sometimes, or never.

2. Direction of Valence: whether he likes to do it, does not like to do it, or does not feel either way about it.

3. Direction of Ego Pressures: whether he

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function of the degree of strain and the perceived probability of strain reduction (p) of the form $F = f[(p - p_0)T]$, affect being positive if the probability of strain reduction is above a critical value (p_0) and negative if the probability is below that value.

feels he ought to do it, should not do it, or does not care.

4. External Pressure by Mother: whether she thinks the child should do it, should not do it, or does not care.

5. External Pressure by Father: whether he thinks the child should do it, should not do it, or does not care.

6. External Adjustment Process with Mother: whether he has disagreements about this with his mother often, sometimes, or never.

7. External Adjustment Process with Father: whether he has disagreements about this with his father often, sometimes, or never. An attempt was made to obtain a representative sample of children's activities in the family, by differentiating the total activity domain of the family into a number of regions and selecting activity items from each. The following regions were used: Household Tasks, Economic Activities, Social Activities, and School Activities which overlap with the family, such as, for example, doing homework. The scale used consists of 34 activity items.

Group administrations were made to all 6th grade children in two junior high schools of a country town in the vicinity of Philadelphia. The total population studied consisted of 128 children of whom 63 were boys and 65 were girls, all living with both their parents.

The following are the operational definitions of situational variables in terms of the method of measurement.

A *Size of the activity domain* or degree of participation measured by the proportion of activities engaged in, giving a value of 1 to activities which are engaged in often or always and a value of ½ to activities which are engaged in sometimes. (Activities which are not available to the child are excluded from the analysis. Thus if the question deals with mowing the lawn, and the family does not possess a lawn then the item is not scored.)

$\bar{L}$ *Degree of negative valence* of the situation measured by the proportion of activities which the child dislikes doing or would dislike doing.

$\dot{L}$ *Degree of positive valence* measured by the proportion of activities which the child likes doing or would like to do.

V *Vitancy* or involvement in the situation measured by the sum of the degree of negative and positive valence.

S *Degree of situational stress* given by the proportion of activities on which an opposition exists between valence and/or ego pressures on the one hand and external pressures on the other.

C *Degree of external adjustment processes* or conflict, given by the proportion of activities about which the child has overt disagreements or quarrels with his parents either often or sometimes.

The question arises at this stage whether in the derivation of situational variables we have kept to the basic set of concepts with which we started off. It will be shown in the discussion of situation dynamics that each of the above variables can be expressed as a function of not more than two concepts; one possible pair of basic concepts in terms of which the remaining variables are most easily expressed are found to be vitancy and stress.

The functional relationship between the above six variables may be stated in terms of a set of four equations which describe the situation system. In the following we shall consider two sets of such equations, one at the first and the other at the second level of approximation. The present data permit a direct test only of the first approximation which is limited to the high vitancy range. The second approximation differs from the first mainly by including the statement that valence values will become zero as vitancy of the situation becomes zero which is given by definition. Throughout, data have been fitted to the second approximation. The behavior of the functions discussed in the lower vitancy range therefore stand at the present stage only as predictions which still require confirmation.

The first principle of situation dynamics

In the introductory section it was stated that in a behavior system the relation between energy (E), strain (T) and level of system functioning (U) should be of the

form

$$T = \alpha E(E - U)^2$$

The relationship can be tested for the case of the situational system by substitution of the corresponding situational variables. We have already coordinated the degree of strain to the degree of negative valence ($\bar{L}$) of the situation and coordinated the amount of energy associated with the system to the degree of vitancy (V) of the situation. For the level of system functioning we substitute the *activity level* (A) of the individual within the situation so that the corresponding principle of situation dynamics becomes

$$\bar{L} = a_1V(V - a_2A - a_3)^2 + a_4 . \quad [1a]$$

The coefficient a_4 has to be added since no single situation can be regarded as completely closed so that an extra-situational contribution to the degree of negative valence has to be taken into account. The level of system functioning of a behavior system has been defined in terms of three dimensions: (*a*) the number of system elements; (*b*) the intensity of element states, and (*c*) the level of integration of elements. In terms of the activity level of the individual these correspond respectively to (*a*) the number of activities engaged in by the individual, (*b*) the intensity level at which they are engaged in, and (*c*) the level of activity organization. In the present study the equation can be directly tested only for the size of the activity domain which is defined by the first two dimensions in terms of the number of situational activities engaged in and the frequency with which they are carried out.

The theoretical equation which represents a family of curves is tested by plotting the degree of negative valence against the vitancy of the situation for size of activity domain values of 44− and 60+ respectively (Fig. 1). The coefficients for fitting the equation are $a_1 = (4.2)10^{-4}$, $a_2 = .508$, $a_3 = 38.8$, $a_4 = 6.9$, which are used to fit both curves. It should be noted that these values correspond to the aggregate of a set of functions whose coefficients will vary from person to person, and presumably may vary also for the same person at different stages of his development.

Present data permit the testing of the hypothesis only for the high vitancy range. Within that range the findings illustrate for a specific situational unit some of the basic properties of behavior systems previously discussed. We note: (*a*) The greater the importance attached to the situation, the higher the equilibrium level for the size of the activity domain which in this context represents the degree of participation of the individual. Assuming a suitable choice of scale coefficients, the equilibrium level is given by the point where the involvement in or importance attached to the situation is equal to the degree of participation and the curve reaches a minimum value.

(*b*) The normal steady state of the system for nearly all children is found to be one in which the involvement in the situation is higher than the normal degree of participation. This supports the view that the maintenance of some degree of strain rather than total strain reduction is a characteristic of normal functioning.

(*c*) If the degree of participation remains constant a reduction in importance attached to the situation meets an initial strain barrier in the form of an experience of increased negative affect.

A decrease in participation will be experienced as pleasant or unpleasant depending on the following conditions: if the existing level of participation is greater than the importance attached by the individual to the situation then a decrease in participation will be experienced as pleasant until the point of equilibrium is reached. To the extent that participation is decreased below the level of importance attached to the situation, the situation will be experienced as unpleasant. In other words, to the extent that the person moves out of a situation either of his own accord or under external pressure, but retains his emotional attachment, he will experience negative affect. This is supported by a study by Farber (5) who shows that the suffering of prisoners remains high to the extent that they retain their attachment to the outside world.

If we restrict ourselves to the upper vitancy range then the third principle can be simplified for the purpose of a first approxi-

mation to the form

$$A = \frac{V - \alpha_1\sqrt{\bar{L}}}{\alpha_2} - \alpha_3 \qquad [1b]$$

showing that under these conditions

1. *The size of the activity domain increases with the vitancy of the situation and decreases with the degree of negative valence.*

Since, again at a first approximation, vitancy is defined in terms of the sum of positive and negative valence strength, the equation may be simplified further to the statement that the amount of activity which the individual engages in increases with the degree of positive affect experienced and decreases with the degree of negative affect. This appears to correspond rather closely to the assumption made in traditional learning theory that performance will increase with reward and decrease with punishment. It appears that the principle in this simplified form has only restricted validity.

On the basis of the derivation of the first principle, the same functional relationships should hold if we substitute the level of performance for the degree of participation. This implies that if we keep both vitancy and valence constant, then an increase in the size of the activity domain can occur only at the expense of performance level and vice versa an increase in performance level involves a decrease in the size of the activity domain.

The second principle of situation dynamics

A state of situational stress is said to exist, if an opposition is found in the direction of external pressures and the internal forces of the individual. For instance, the child may dislike doing a household task, feel he should not do it, but perceive his mother as wanting him to help. It may happen that an internal opposition of forces exists, where the child dislikes the activity but feels he should do it. In this case, an external pressure in either direction will imply a state of situational stress.

The existence of a state of stress implies a displacement of the system away from its equilibrium state which, by definition, is equivalent to a state of strain. Since one possible route toward strain reduction lies in reducing the strength or direction of opposing pressures, we may state, to begin with, that the degree of situational stress will be directly related to the *degree of external adjustment processes.*

Suppose now that no external adjustment processes can occur. This condition may be approached, although never completely achieved, if the parents exert sufficient pressure to prevent attempts by the child to change their behavior. In this case, a state of strain exists without available means for strain reduction. But it is postulated that a state of this type will be perceived to have a negative valence. It follows that for a given degree of situational stress (S) an increase in the degree of external adjustment processes (C) will lead to a decrease in the degree of negative valence ($\bar{L}$). The second principle of situation dynamics may then be stated in the form

$$S = f_1(C) + f_2(\bar{L}). \qquad [2a]$$

Figure 2 shows the relationship obtained between the degree of negative valence and the degree of external adjustment processes for situational stress ranges 0–14, 15–44, and 45+. It will be seen that the greater the degree of situational stress the greater the joint degree of external adjustment processes and negative valence. At the same time for a given range of situational stress, adjustment processes and negative valence are inversely related. Assuming a linear relationship[6] the equation becomes

$$S = \frac{C + b_1\bar{L}}{b_2} - b_3 \qquad [2b]$$

where $b_1 = .712$, $b_2 = 1.17$ and $b_3 = 21.6$, which are used to fit 13 points given by the data.

Since external adjustment processes in-

[6] A better fit is given by a family of hyperboles. A nonlinear relationship would however require a change in the remaining system equations making these rather more complex. The linearity assumption maintains consistency and provides correct predictions at both the first and second level of approximation. It will very likely have to be modified when improvement in measurement techniques makes the testing of more complex and accurate functions feasible.

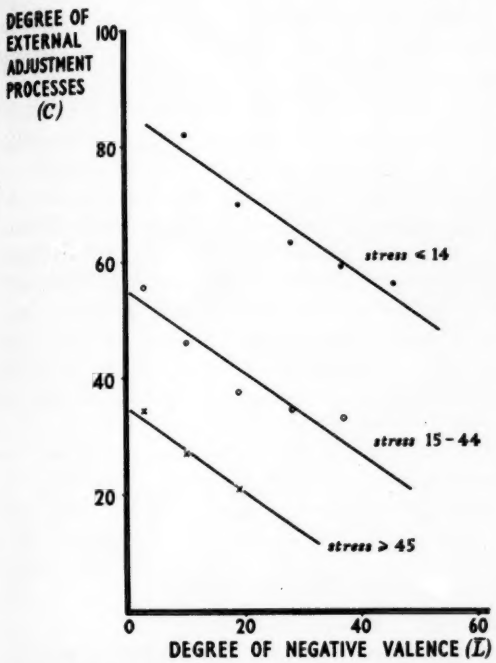

FIG. 2. Relationship Between External Adjustment Processes and Negative Valence of the Situation with Stress Constant (Mean S = 8.3, 24.8 and 52.2 respectively).

The theoretical relationship is given by Eq. 2b.

clude what are denoted as aggressive reactions by Dollard *et al.* (3), the principle includes the frustration-aggression hypothesis as a special case. Empirical studies show that aggression is only one of a number of possible external adjustment processes which the individual may resort to under conditions of stress. In their experiment Barker, Dembo, and Lewin (2) found that children who were deprived of attractive toys attempted to make the experimenter raise the barrier which separated them from the toys or tried to get through or remove the barrier by themselves. The approaches to the experimenter were found to range all the way from coaxing, reasoning, hostile remarks to the attempted use of force. At the same time the children displayed increased negative emotionality.

Further empirical support for the principle is found in experiments by Leavitt (14) and Shaw (19) on patterns of communication channels. Both experiments show that the less facility the person has to communicate to other members of the group, as determined by his location in the communication net, the greater dissatisfaction he experiences with his task. The findings are confirmed in a study by Hare (6) on the effect of group size on communication. An increase in the number of members of discussion groups decreases the average communication potential of all members which, in turn, is found to result in increased dissatisfaction with the group discussion. The relationship at the level of group functioning would be expected to hold only in the absence of coalition formation since this process is equivalent to a reduction in the number of group members in terms of the number of factions of which the group is composed.

In a different context, Roethlisberger and Dickson (18) found that employees who were given an opportunity to talk about stress and conflicts they experienced in their work, expressed relief after getting things talked over. It should be pointed out that the interviewer could not assist them in producing external changes. The same cathartic process would appear to be involved to varying extent in most forms of psychotherapy. An increase in external adjustment processes decreases experienced internal tensions irrespective of their actual success, a fact which may explain behavior which might otherwise be regarded as irrational. Among clinical findings a discussion of jealousy by Riviere (12) is relevant. She finds that "the more furious and aggressive [the jealous individual], the less humiliated he feels, and vice versa the less aggressive and angry, the more miserable and low spirited he feels."

An actual reduction of strain can occur under three conditions. External adjustment processes may result in a reduction or change in direction of opposing external pressures. Alternatively, a change in internal forces may occur to conform with the direction of external pressures, a process referred to as *internalization*. The second principle shows that in either case a joint reduction in external adjustment processes and degree of negative valence of the situation will result. The third process which we shall return to

later involves a reduction in vitancy, that is, a withdrawal of energy from the situation.

Although each process achieves a reduction in strain, they differ in their final outcomes which we shall briefly consider. As a first approximation the vitancy (V) of the situation has been operationally defined in terms of the proportion of activities which do not have a neutral valence. This is equal to the sum of negative and positive valences, so that

$$V = \bar{L} + \dot{L}. \qquad \text{[Definition 2]}$$

It follows in conjunction with the first principle, that if reduction of strain occurs at a high level of vitancy, the situation changes directly from one that is strongly negative to one that is strongly positive. If on the other hand a reduction of strain occurs as a result of reduction of vitancy, then the final state is one of neutral valence, and a more or less apathetic conformity to external pressures.

In order to determine the method of strain reduction which will be used, we need to take into consideration the effect of structural changes within the given situation on the strain balance of the individual's behavior system as a whole.

If the various situations existing for the individual are relatively independent of another, internalization and conformity with external pressures within each may occur without resulting in internal conflict. We frequently speak of individuals of this type as having "water-tight compartments," a term, which rather well describes the corresponding system structure. If on the other hand the behavior system of the individual is highly interdependent, then internalization of opposed external pressures in order to reduce situational strain in different situations will result in an increase of internal strain. Conformity and submission to external pressures which would lead to a reduction of strain within one situation may then result in increase of strain within other situations and will, therefore, be blocked in favor of other paths towards strain reduction. The application of strain balance analysis to group functioning and its use for the prediction of social change is discussed in an earlier article (9).

The third principle of situation dynamics

The second principle shows that increasing situational stress will increase the rate of external adjustment processes and the degree of negative valence of the situation but does not enable us to state the extent to which either one will occur. The third principle will be concerned with determining the conditions under which an increase in situational stress will result in an increase in external adjustment processes or in the degree of negative valence respectively.

The fact that the individual is represented as an open behavior system implies that he can achieve only partial control over the outcome of his actions. He can increase his control over his environment and over himself and thus reduce his margin of uncertainty. He can never completely abolish uncertainty over the outcome of an action. The extent to which the person will be concerned over the outcome of an action will be a function of the degree of vitancy of the situation.

To begin with, we assume that external adjustment processes will increase with vitancy. Suppose that the vitancy of the situation becomes increasingly high, then the outcome of an action may be either highly positive or highly negative. An increase in vitancy will, therefore, increase both the painfulness of potential failure and the gratification of potential success, and thus increase the strength of choice conflict. An increase in choice conflict in turn implies an increased resistance against choice of alternatives as shown for instance by increased decision time (1) or in extreme cases an absolute refusal to make an action decision (17). Under these conditions approaching action paralysis, strain reduction by means of external adjustment processes necessitates a reduction in the vitancy of the situation. If vitancy reduction is too sudden, the person feeling that the outcome is not really so important, a sudden increase in pressure toward action results, leading to a precipitous decision. A reduction of vitancy below the critical point on the other hand, leads to

a reduction in pressure toward action, the issue being perceived as less relevant. Absence of action may thus result both from a condition of relatively low vitancy and hyper-vitancy.

The second principle at a first level of approximation may now be stated in the form

2a) *For a given degree of situational stress the rate of external adjustment processes increases with vitancy up to a critical point after which it declines.*

The simplest function which satisfies these conditions is

$$\left(\frac{\delta C}{\delta V}\right)_s = k - V. \qquad [3a]$$

With the help of the second principle we can determine the corresponding relationship between vitancy of the situation and the degree of negative valence.

Differentiating the second principle with change of scale

$$\left(\frac{\delta \bar{L}}{\delta C}\right)_s = -1$$

and multiplying the two equations

$$\left(\frac{\delta \bar{L}}{\delta V}\right)_s = V - k$$

so that

$$\bar{L} = (V - k)^2 \cdot f(S). \qquad [3b]$$

The equation states

2b) *For a given degree of situational stress the degree of negative valence decreases with vitancy up to a critical point, after which it increases.*

The first approximation appears to be sufficient for practical purposes provided we deal with life situations of the individual and issues which are relevant for him. It does not hold for the lower vitancy range. At a second level of approximation we need to take into account that as vitancy approaches zero value, valence forces will drop to zero also, at least in the ideal limiting case where the situation can be looked at as a closed system. The simplest type of function which fulfills the additional requirement is

$$\bar{L} = V(V - k)^2 \cdot f(S) \qquad [3c]$$

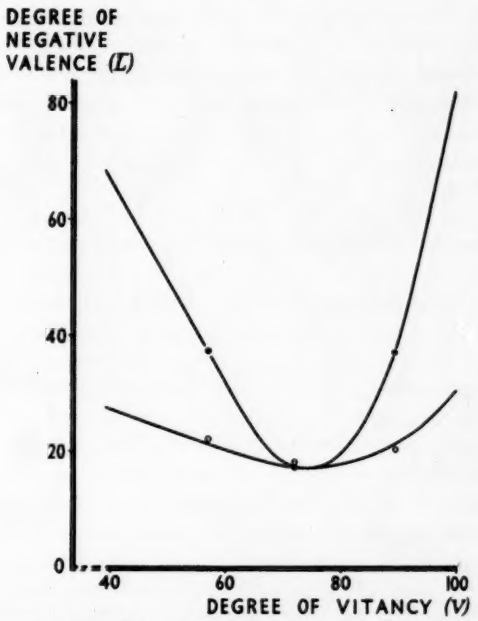

FIG. 3. Relationship Between Negative Valence and Vitancy of the Situation with Stress Constant Within the Range 44− for the lower curve and 45+ for the upper curve. (Mean S = 22.1 and 52.2 respectively). The theoretical relationship is given by Eq. 3d.

which had been used to fit the data (Fig. 3) the equation being

$$\bar{L} = c_1 V(V - c_2)^2 S^2 + c_3 \qquad [3d]$$

where $c_1 = (1.79)10^{-4}$, $c_2 = 75$ and $c_3 = 17$ The first approximation then becomes

$$\bar{L} = \beta_1 (V - \beta_2)^2 S^2 + \beta_3. \qquad [3e]$$

The fact already noted that vitancy values do not fall below a minimum value suggests that there is a critical value below which vitancy of a group situation cannot fall if the person is to remain an effective group member. The minimum value is found to lie between 40–45 for the child as a member of the family in the sample studied. It is likely to be rather lower at the stage when the child begins to move out of his family or for participation in informal groups.

THE SITUATION SYSTEM

The three principles of situation dynamics together with the operational definition of vitancy provide a set of relationships within which each variable can be expressed as a

function of any other within the set. In the present paper both a first and second approximation to the relationship between situational variables have been formulated. The former applies to situations in which the individual is relatively highly involved; the latter seeks to cover the total vitancy range and still remains to be tested directly by a study of situations in which the person is less involved.

It should be pointed out that no importance can be attached to the absolute values of the numerical coefficients which are based on a population sample. Coefficients such as for example the rate at which external adjustment processes increase with stress, the degree of negative valence remaining constant represent characteristics of the individual which will differ from person to person. At this stage of methodological development it can only be pointed out that a theory of situation dynamics whatever its ultimate form at the same time contains a meaningful set of personality concepts and thus provides a basis for the development of techniques for measuring total personality structure. We may note a similar development in the theory of physics where the formulation of general principles goes hand in hand with the discovery of specific coefficients which provide a set of measures for the physical characteristic of an object.

A more immediate problem is that of checking the internal consistency of the relationships which have been formulated. In order to test this we shall need to use the set of equations for the purpose of prediction. It will not be possible here to discuss the rather large number of new behavior principles which may be derived as theorems. Instead, we shall choose as a test the solution of a problem which has some special interest and on which empirical evidence is available.

The effect of stress on performance level

In their experiment Barker, Dembo, and Lewin (2) showed that frustration led to a reduction in the level of constructiveness of play in a large number of subjects but there were some who under the same conditions showed an increase in the constructiveness of play. This is in agreement with common observation. There are individuals who appear to be stimulated by conditions of stress and produce a more effective performance while others show behavioral disorganization and a reduction in the effectiveness of their performance. Clearly, there is at least one additional variable involved which determines the outcome of a strain-producing situation. The problem we want to solve is, what is the additional variable which determines whether an increase of situational stress will lead to an increase or decrease in performance level, and what is the functional relationship between the variables involved.

From the first and third principle, Equation 1b and 3d, by eliminating the degree of negative valence and setting $c_3 = a_4$ (since both coefficients correspond to the extra-situational contribution to the degree of negative valence apart from errors involved in the use of mean values for a set of functions) we obtain

$$c_1(V - c_2)^2 S^2 = a_1(V - a_2A - a_3)^2.$$

Taking the square root of both sides, rearranging and simplifying coefficients

$$A = \frac{(S - 1_1)(1_2 - V)}{1_3} + 1_4 \qquad [4a]$$

giving the relationship between size of activity domain and degree of stress. Figure 4 shows the relationship between size of activity domain and stress for vitancy ranges 70– and 86–100. The data are found to be in accord with the derived function. It may be noted that the same functional relationship is obtained if we use the first approximation to the first and third principle (Equation 1c and 3e).

The additional variable which we were looking for is found to be the vitancy of the situation. The relationship for the normal stress range may be stated in the form

4 a) *To the extent that the vitancy of the situation is above a critical value, the size of the activity domain decreases with situational stress.*

4 b) *To the extent that the vitancy of the situation is below a critical value, the size of*

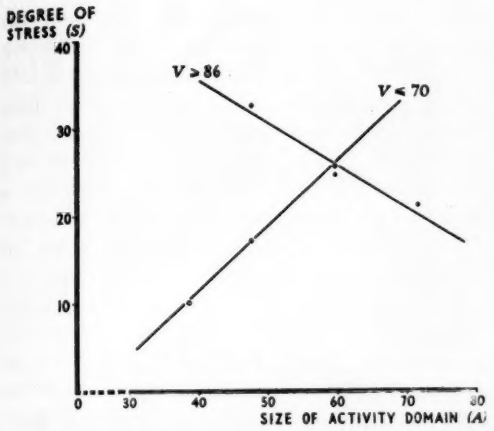

FIG. 4. Relationship Between Stress and Size of Activity Domain for Vitancy Ranges 70— and 86–100 respectively.

The predicted functional relationship is given by Eq. 4a.

the activity domain increases with situational stress.

If we substitute level of performance for size of the activity domain we find that a high level of performance will occur under two distinct conditions.

1. The person is highly involved in the task, situational stress is low, and consequently the task is experienced as enjoyable.

2. Task involvement is low, situational stress is high, and consequently the person gains little pleasure from his achievements. Similarly, a low level of performance will occur under two distinct conditions:

1. The person is highly involved in this task, stress is high, and the situation is experienced as highly unpleasant.

2. The task is perceived as being of no importance, no stress operates, and consequently no satisfaction or dissatisfaction is associated with the task.

The view that learning and effectiveness of performance are a direct function of motivation has to be modified. The relation holds only under conditions of low stress. Under conditions of high stress, on the other hand, a reduction of motivation below the critical vitancy value is required to increase the effectiveness of performance.

The experienced affect associated with each condition under which performance may occur is derived from the second principle and the operational definition of vitancy in terms of valence strength. If, for instance, stress is low, then we know from the second principle that the degree of negative valence is low. If at the same time vitancy is high then since vitancy is a function of the sum of positive and negative valence it follows that under these conditions the degree of positive valence must be high so that the task will be experienced as enjoyable.

Individuals will differ with respect to the critical vitancy value beyond which their activities become disorganized with increased stress. Suppose we divide our population into a group with low critical values and a group with high critical values, then if individuals in both groups are compared under conditions of high and low stress, the former will show a reduction in the mean performance level, while the latter will show an increased performance level. The present study shows that the critical value beyond which stress leads to deterioration of performance should be identical with the critical value beyond which stress leads to a reduction in external adjustment processes and increased negative valence; $V = 72.9$ and 75 respectively. We may, therefore, use readiness of individuals to react to stress with negative emotionality, such as failure experience, humiliation, pessimism, and self-blame as a measure of their critical vitancy value beyond which stress leads to performance deterioration. A measure of this type has been used by Waterhouse and Child (20) who find in fact that individuals with high readiness for negative emotional reactions under stress (low critical vitancy value) show a reduction in mean performance level under stress, while those with low readiness for negative emotional reactions (high critical vitancy value) show an increase in mean performance level.

Transformation of reference frames

The discussion of behavior principles has been concerned with the properties of a unitary system with respect to its environment. The system may be a single situation existing for the individual or the total behavior system of the individual treated as an undifferentiated whole. The next step is to study

the behavior system of the individual as a differentiated whole with the various situations existing for the person as component elements. In the study of an adult in our society, we would need to consider, for instance, his participation in his present family and in his family of origin, his participation in job activities, religious and political activities and his participation in peer groups and associations. The measurement of the total behavior system of the individual requires a measurement of each of these situations and a determination of their interrelationship. Since the interdependence relationships between situations can be represented as a set of coefficients relating situational variables, no additional variables need to be introduced in passing from the study of a single situation to a study of the total behavior system of the individual.

A change in the frame of reference is required if we use a group instead of an individual as our unit of analysis. The same system principles should, however, continue to apply.

It has been shown that within any behavior system three types of forces operate on the components of the system; these are (*a*) forces deriving from the components, (*b*) forces deriving from the system, and (*c*) forces that have their source in the environment of the system. In the case of the individual these were seen to correspond to valence, internal pressure and social pressure respectively. If we now take as our unit of analysis a group of which the individual is a member, then the individual who was previously looked at as a single system now takes on the role of a component within the behavior system of the group. The forces operating within the group with respect to its members as components will be (*a*) forces deriving from members, (*b*) forces deriving from the group as a system, and (*c*) forces having their source in the group's environment. These are clearly the same forces which we have been considering before only differentiated from one another in a different way.

In passing from the individual to the group as a unit of analysis, the joint component and system forces of the individual now become the component forces of the group, while the social pressures operating on the individual now become split off into the system forces of the group and those having their source in the environment of the group. If an institution is defined as a set of interdependent groups, then by a similar shift to the next higher unit of analysis we arrive at the forces operating on groups as components of an institution.

The transition from one unit of analysis to another within the hierarchy of behavior systems is seen to involve a change in the frame of reference in the conceptual representation of behavior events, but not in the type of phenomena included in the analysis.

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Three Views of Mathematics

(Quoted in *Mathematics in Western Culture* by Morris Kline)

The good Christian should beware the mathematicians and all those who make empty prophesies. The danger already exists that the mathematicians have made a covenant with the devil to darken the spirit and to confine man in the bonds of hell.

—St. Augustine

Mathematics, rightly viewed, possesses . . . supreme beauty—a beauty cold and austere, like that of sculpture, without appeal to any part of our weaker nature, without the gorgeous trappings of painting or music, yet sublimely pure, and capable of stern perfection such as only the greatest art can show. The true spirit of delight, the exaltation, the sense of being more than man, which is the touchstone of the highest excellence, is to be found in mathematics as surely as in poetry.

—Bertrand Russell

When a twelfth-century youth fell in love he did not take three paces backward, gaze into her eyes, and tell her she was too beautiful to live. He said he would step outside and see about it. And if, when he got out, he met a man and broke his head—the other man's head, I mean—then that proved that his—the first fellow's girl was a pretty girl. But if the other fellow broke *his* head—not his own, you know, but the other fellow's—the other fellow to the second fellow, that is, because of course the other fellow would only be the other fellow to him, not the first fellow, who—well, if he broke his head, then *his* girl—not the other fellow's, but the fellow who *was* the—Look here, if A broke B's head, then A's girl was a pretty girl; but if B broke A's head, then A's girl wasn't a pretty girl, but B's girl was.

—Jerome K. Jerome

THE ROLE OF CONVULSIONS IN BEHAVIOR[1]

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Are convulsions always indicative of pathology? There is evidence that sometimes convulsions may be instances of adaptive behavior, as, for example, those associated with the falling posture in mice. This form of convulsion seems to protect the falling animal from hemorrhages of concussion. Certain types of convulsions, such as audiogenic seizures in the Peromyscus, are of genetic origin so that strains of mice subject to such seizures can be bred. This leads to the conclusion that certain convulsions may be regarded as parts of the "behavioral repertoire" of an animal which in certain circumstances may become adaptive.

EVEN though wartime experience did much to extend the concept of "petit mal," and so to include within pathology minor aberrations in the field of coordinated behavior, until recently the occurrence of convulsions in the behavior of apparently healthy individuals had not been suspected. The accepted view in medicine has been that convulsions are a manifestation of pathological processes, and this attitude has been carried over without question to the study of similar phenomena in animals. Evidence of "grand mal" convulsions in the orgastic behavior of a proportion of normal adolescent and adult males (21) makes it clear that this is not necessarily so. This new evidence demands an assessment of the place of convulsions in the whole range of behavior, both normal and abnormal, and suggests the possibility that convulsions might play a role in adaptive behavior, if not in ourselves, then at least in animals. Such a possibility falls outside the scope of medicine, which, with the accent of research on the pathological, has been mainly concerned with differentiating the various pathological origins of the seizure in epilepsy. The biological approach, however, must attempt to define the place of this behavior in the whole repertoire of activities, and must take note of any evidence for a possible function for convulsive behavior in the behavior of any animal, including man.

To do this, however, the concept of normal and abnormal must be set aside for this stage of our inquiry. The concept has too many connotations to be useful. Reverting instead to the touchstone of all biological inquiry, we are impelled to ask, "Are convulsions ever an integral part of adaptive behavior?" This would imply that the brain has a mechanism for the expression, i.e., initiation and control, of the type of nervous discharge which is found in convulsions. If this is definitely so for some animals, then the mechanism may exist, if not performing an adaptive role in human beings, then as a vestigial mechanism from some earlier stage in evolution. Behavior that is adaptive in one animal is not necessarily so in others, but an understanding of the various manifestations of convulsions can provide us with the basis for distinguishing the pathological from the nonpathological features. In considering the evidence I shall bring forward examples from many different fields of study.

The convulsive brain glycogen increment of the mouse

Chance and Yaxley (5) found that during a convulsion the glycogen rose in different parts of the mouse brain. Following a variety of convulsive stimuli the rise was largest in

[1] It was a pleasure to receive comments from W. M. S. Russell, to whom I am indebted for ideas incorporated here and from W. Bevan.

the cortex (6). The increment was a specific attribute of the convulsion and not of the stimulus, since stimuli at subconvulsive intensities, sufficient to cause excitement in the mouse, failed to produce an increment, whereas whenever a convulsion was produced an increment was found.

The stimuli included, first, lack of oxygen. When oxygen and nitrogen are passed over the animal, the oxygen being subsequently withdrawn gradually, the animal dies quietly after a short period of excitement, with a small increase in brain lactate and no increment in glycogen. If, however, 12.5% of carbon dioxide, which by stimulating the respiratory center enables respiration to continue at low oxygen tensions, is mixed with the nitrogen, the withdrawal of oxygen leads to clonic convulsions, lasting five to twelve seconds. The increment in lactate is larger than without the carbon dioxide present and there is an increment of approximately 30% in glycogen.

A similar result is obtained by inducing convulsions through the injection of drugs—in these experiments the intravenous injection of Metrazol, picrotoxin, amphetamine, and nikethamide. Animals which convulsed at the CD 50 dose of the drugs showed glycogen increments of about 60% with increments of lactate which were larger than those obtained when the same drugs were given at two thirds of the CD 50 and did not produce convulsions or an increment in glycogen.

Finally, electrical stimulation was used. The subconvulsive stimuli in this instance induced a tetanus (i.e., rigidity of the limbs and trunk muscles in the positions in which they are when the stimulus is applied and lasting only the duration of the stimulus) but this had no effect on the glycogen, although, as might be expected, the lactate rose. In these experiments moreover, it was possible to separate the effect of the stimulus from the effect of the convulsion, since an interval elapsed between the end of the tetanizing stimulus and the onset of the convulsion. If the animals were killed before the convulsion, a second or so after the stimulus had been removed there was no increment in glycogen.

Thus, there is evidence that brain glycogen increments in the mouse are an indication of convulsive discharge. A corollary of this would be that if the convulsions were more intense the glycogen increment would be greater. On the assumption that clonic (i.e., alternating flexor and extensor spasm) convulsions are less intense than tonic (i.e., extensor rigidity of all the body musculature) convulsions, a fact that can be deduced from the observation of convulsions in a variety of situations, a greater increment would be expected with tonic than with clonic. This was in fact so. The increment was not much greater but it is statistically significant in all four parts of the central nervous system: the cortex, the midbrain, the medulla, and the spinal cord.

The behavior of brain glycogen was then examined in mice of a number of strains showing an idiopathic susceptibility to convulsions. Convulsions in the mice of these strains can be produced by specific external stimuli. Stimuli reaching the central nervous system via the receptors will be referred to as *intrinsic* stimuli because they are dependent upon the functioning of a built-in afferent mechanism, in contrast to *extrinsic* stimuli which include drugs and electrical stimulation. An extrinsic stimulus is so called because it produces its effect directly on the cells of the central nervous system, and not through the alteration of the pattern of stimuli reaching the receptors. Two strains in particular have been examined—the strain of Peromyscus which shows a typical audiogenic seizure (4), and the strain of H.C. mice, or hanging-convulsion mice, which show a convulsion when held up by the tip of their tail and jerked (8). Both stimuli can be given at subconvulsive and convulsive intensities. The results confirmed the earlier findings that brain glycogen increments are a specific reflection of convulsive activity in the central nervous system; convulsive activity in both contexts being recognized by clonic and/or tonic muscular movements.

The falling posture

The posture of the hanging convulsion in the HC strain of mice had been seen during

convulsions produced by Metrazol and picrotoxin, and appeared to be an entity representing a specific central nervous function. It was also noticed that as a mouse fell it adopted a similar posture. Flash photographs of mice thrown into the air showed that the mouse adopted the same posture as that of the hanging convulsion (8). Careful examination of the photographs revealed that the mouse adopted this posture as soon as it reached the top of its trajectory and that, in fact, it was the posture adopted by falling mice.

It is a striking fact that if you allow a mouse to fall from a height of 8 or 10 feet onto a wooden floor, it rarely damages itself. This is very probably the result of the rigidity of the limbs and body which can be felt in the mice which show the hanging convulsion. The position of the limbs is undoubtedly important as they are extended forward and downward, and in the case of the hind limbs, outward from the body. It appears, therefore, that the posture adopted during the fall, for a period of less than half a second, enables it to take up the shock without damage. In order to prove this point we compared the effect of a fall on five normal and five anesthetized mice, and found that hemorrhages occurred in all of the anesthetized mice, whereas there was only one lesion in one of the mice allowed to fall unanesthetized (8). This type of behavior is, therefore, adaptive, preventing the hemorrhages which arise from nodes of concussion. These concussion nodes appear to develop since the lesions occur at special points on the body.

Brain glycogen increments and the behavior of the mouse

We therefore examined the brain glycogen of normal mice following a fall, and discovered that there was an increment after a fall in the normal mouse which was smaller than in the idiopathically susceptible strain dropped from the same height.

To enlarge the picture further, the brain glycogen in a number of different forms of behavior was examined. These included running, swimming, jumping, a fall with or without the righting reaction, the defensive and offensive roles in a fight, and sleeping. No increases were demonstrated during sleep, after running, swimming or after defensive behavior in a fight. But a jump, a fall, and aggressive behavior in a fight produced significant glycogen increments.

The extent of the rise in the different types of behavior was 60% in the cortex following a fall of 8 feet, 20% following aggressive fighting behavior which was allowed to proceed to the end of a fight, and 8 to 9% following a jump of 4 inches. This suggested two possibilities; namely, that these increments represent convulsive discharge or that glycogen is in fact raised in the brain during nonconvulsive types of behavior as well as during convulsions. The widening of the picture to include different types of behavior seemed, at this stage, to have blurred the outlines of the original findings, so further investigations of a rather different kind were undertaken.

Since the behavior producing glycogen increments was evoked by intrinsic stimuli, it was natural to inquire into the role of the entero-receptors which might be responding to changes in the internal environment. Since glycogen is derived indirectly from the glucose of the blood and since the mouse brain utilizes sugar as its main source of energy (R.Q. = 0.89), it is reasonable to assume that examination of the physiological factors influencing blood glucose would throw some light on the factors in the internal environment which might be responsible for the change in brain glycogen brought about by some types of behavior. By extending the investigation in this way we hoped to find out whether any physiological mechanisms were responsible for any of the glycogen increments found in the brain following particular types of behavior. The result of this investigation (10) shows that any factors which cause a rise in blood sugar, such as adrenalin or the injection of glucose, do bring about a small, but statistically significant increment in brain glycogen. This is about 10% for a blood sugar rise of 20% such as is encountered in fighting behavior. One other factor is also responsible for an increment; namely, the anti-insulin hormone of the anterior pituitary. This pro-

duces a slightly smaller increment of approximately 7% in a massive dose, which is also statistically significant in these experiments. It was shown that these two factors working together could produce an increment of about 14%. We can now say, therefore, that the increments found after a jump and after dominant behavior in a fight might reasonably be the result of internal physiological changes. This is the more probable since, following dominant behavior in a fight, there is a significant rise (+19%) of blood sugar, but there is no change following submissive fighting behavior. However, it must be noted that a rise of blood sugar is not invariably followed by a rise in brain glycogen, since following running a significant blood sugar rise of 14% occurred unaccompanied by a rise in brain glycogen. Moreover, the reverse is also true since the blood sugar remains unaltered immediately following a fall. Thus, it is clear that the large glycogen increment of a fall is independent of a change in blood sugar, and is too large to be accounted for by the action of the anti-insulin hormone even if, as is unlikely, it could act in a matter of a few seconds. We are entitled to conclude, therefore, that within the scope of the investigations into the physiological factors, no contributory factor in the internal environment capable of accounting for the glycogen increment of a fall, has been revealed. The evidence, therefore, linking a major glycogen increment to convulsive activity in the brain of the mouse remains valid. The inference is that because a large glycogen increment is found in association with an adaptive form of behavior in mice, namely the rigid falling posture which protects them from damage, we have evidence that a "convulsive type of brain activity" underlies one type of adaptive behavior.

The nature of the convulsion in the falling mouse is similar in many features to a convulsion produced by extrinsic stimuli. The change in glycogen is widespread throughout the central nervous system following both types of stimulus, being greater in the cortex than the medulla and brain stem, and even smaller in the spinal cord. The pattern of motor activity, therefore, is accompanied by changes in a very large volume of nervous tissue, and would appear to be different from a focal discharge. The tonic musculature of the falling mouse is by all appearances the same as that of the muscles in a tonic convulsion, but superimposed upon this is a different posture.

The features of convulsive activity in the brain

Convulsions have hitherto been recognized by simultaneous vigorous movement in all parts of the body, hence their distinctive quality. So striking is this vigorous movement that it has been tacitly assumed to be the essential quality, but from what has been said about the falling posture and the behavior of brain glycogen during a fall, it is clear that vigorous *movement* should no longer be considered the only criterion by which to judge whether or not a convulsion has occurred. The essential quality lies not in the form of movement so much as in the nature of the changes taking place in the brain. These, as far as the evidence on brain glycogen shows, are distinctive. It now remains to show how far other changes in brain tissue can help to confirm the distinctive quality of convulsive discharge.

Attempts to identify by histochemical means the locus of brain glycogen revealed an increase of Feulgen staining in the brain tissue nuclei of both the granular and molecular layers of the mouse cerebellum following convulsions produced by picrotoxin and Metrazol, as well as after a fall. Further investigation revealed, however, that this increase in staining was a consequence of a contraction in the size of the nuclei (11), and was not accompanied by any change in the amount of the stained material (DNA). Following a fall the area of the nuclei declined to a mean value 40% below that of nuclei from resting mice. Following drug-induced convulsions (picrotoxin and Metrazol) the area fell to approximately 60% of that in the resting mouse. In contrast, following different forms of vigorous movement such as swimming and running the area of the nuclei increased to approximately 140–150% of the resting value. These changes in nuclear size also suggest that convulsive dis-

charge is a distinctive type of neuronal activity.

The biological framework of behavior studies

The evidence that has just been presented shows that one form of convulsive behavior (the falling posture of mice) is part of adaptive behavior. In order to raise the question, "Are other forms of convulsive behavior found in mammals, pathological, as assumed to date, or are they functional?" certain postulates must be discussed.

A biological function can be ascribed to an act if it can be shown to be part of a sequence or pattern of behavior which leads to the procreation of the next generation. This can be achieved either directly by participating in the mating sequence or the care of young, or indirectly by facilitating the survival of the animal until it reaches maturity. Acts thus serve a biological function if thereby the animal satisfies one of the various biological "needs." These for a mammal are:

I. For maintenance in health
- (a) Supply of essential material
  1. Supply of food
  2. Supply of water
  3. Supply of oxygen
- (b) Elimination of residues and metabolites
  4. Excretion of water
  5. Voiding of waste
- (c) Control
  6. Loss of metabolites
  7. Regulation of body temperature
  8. Maintaining bacteriostasis
- (d) 9. Survival under stress.

II. Reproduction

To subserve a need, different types of behavior acquire different degrees of biological efficiency. By efficiency, the biologist means the frequency with which a need is provided each time the act is performed. Some types of behavior possess high efficiency in most circumstances. All of the survival needs are met by behavior with high levels of efficiency, e.g., drinking. Others usually possess a high efficiency except when the environment is deficient in the essential material, e.g., feeding and breathing.

In social relations, however, which subserve a number of different needs this efficiency is sometimes much less. For example, courtship behavior, in birds especially, need not only always succeed, since the acts of courtship require only a single successful mating sequence for procreation to take place.

Here "displacement, vacuum and redirection activities" (2) serve an intermediary function since by providing certain operational requirements of the particular nervous system, they facilitate ultimate efficiency of the behavior. Thus, the concept of biological efficiency defined in relation to the individual animal is related ultimately back to our ability to show whether or not a particular type of behavior can facilitate the survival of the animal or its powers of breeding. Finally, we must find out whether the behavior can be identified in the wild before deductions about these matters from laboratory observations can be accepted as evidence.

The criterion of survival, however, is sometimes not so much the individual as a unit of population and if we relate the occurrence of a particular type of behavior to this wider biological unit, it may be possible to ascribe a functional significance to the repertoire of behavior shown by only a proportion of a wild population. The criterion of survival although ultimately referable to the life of an individual, may nevertheless be reflected in the continuation of a species by the survival at any one instance of animals showing one or another type of alternative behavior patterns. For if so, it may be sufficient to show that alternative behavior patterns have selective advantage in special circumstances and may have become established in the hereditary foundation of the animal's behavior on clearly defined evolutionary principles. A low incidence behavior pattern may, moreover, be adapted for the survival of relatively few individuals in extreme conditions which may appear at long intervals on rare occasions in conditions of life of members of a population, or of the population as a whole.

As was shown in the previous section, the full range of an animal's behavior will not be manifest until we are aware of the full range of stimuli and situations to which it is subject in the wild state, and we are far from possessing that degree of knowledge which can give us the assurance that this is or is not so, even for such an economically important animal as the rat. Studies of this kind are even now only in their infancy and are not yet fully directed toward this goal. Even when this aspect of the behavior potential has been fully investigated, we have yet two other features to evaluate. The first arises out of the normal variability of a population which underlies the individual differences in behavior of each member of a group and which may cause different individuals to give quite different reactions to the same situations. It is also the reason why some reactions are restricted to only a few individuals. The normal adaptive repertoire of a population may involve reactions which are only encountered occasionally in a population and may be hardly ever noted if the necessary stimulus situations (owing to the restricted attention and methods of the investigator) are rarely present. There is also the influence of the evolutionary past in determining the presence of features which may be occasionally present and yet which may have no significant adaptive function in present circumstances. Finally, there may be elements of behavior due to the properties of the nervous system per se as at present constituted which may be no more than tolerated aberrations, insufficiently interfering with efficiency to have been selected out.

"Audiogenic seizures" in rodents

Apart from the well-known fact that toxic substances and high doses of stimulant drugs induce tonic and clonic movements in rodents, similar in many ways to grand mal epilepsy in man, animals showing "fits" have been occasionally encountered in many laboratory stocks of animals. If, however, without adequate assessment, unusual (i.e., infrequently encountered under present laboratory conditions) types of behavior in animals are regarded as pathological, the essential nature of that behavior may remain obscure, but just this mistake has been made in the study of audiogenic seizures. Apparently the mere superficial resemblance of the "fits" to clonic seizures in man has been deemed sufficient ground to classify the phenomenon as "pathological" or "abnormal," though "sudden" or "explosive" characters are other features advanced for use of these terms. There is no doubt that this attractive possibility has done much to encourage the belief that audiogenic seizures are similar in most respects to epilepsy in man, and therefore essentially pathological phenomena. In this way investigators have started with preconceived ideas which exclude the possibility that audiogenic seizures might be a form of adaptive behavior.

As Michels and Bevan (25) point out, "Over a decade's work on 'audiogenic' seizures has provided a large number of research reports, at least one vigorous controversy, but only limited insight into the mechanisms underlying these reaction patterns." The need for a radically different approach is, therefore, overdue and should be welcome.

These assertions about the pathological nature and similarity to epilepsy in man of convulsions in animals have caused an increasing amount of work on the occasional animal and its descendants, which have been noted from time to time in experimental stocks. Donaldson (14) referred for the first time to maniacal running, jumping and final exhaustion in rats subjected to the sound of jingling keys. These running fits or "audiogenic seizures" have since been studied in a large number of laboratories, yet they remain a biological curio, uninterpreted so far in terms of fundamental biological concepts.

This type of behavior has been recognized not only in rats, but also in mice, guinea pigs, Peromyscus, and latterly in the rabbit. In mice they may appear at any age, dependent on the strain, up to or just beyond maturity, but in Peromyscus they are confined to immature animals. Moreover, they have been found only in laboratory stocks, though in Peromyscus they appeared in the first generation removed from the wild. A detailed summary of the experimental work is contained in Munn (28), from which the follow-

ing description of the behavior during audiogenic seizures in rats is taken:

> After the initial startle response to the onset of the stimulus, any of a number of patterns may appear. Some animals appear to ignore the sound source, others seek to retreat from it, a few actively attack it. Many rats crouch motionless or try to burrow beneath the floor covering or their fellow subjects. In most non-seizure trials some sort of "substitute behavior" appears—rapid nose or ear rubbing, teeth chattering, chewing, shivering, shaking, vibrissae twitching, body cleaning, or restless head and body movements. In trials culminating in a seizure, behavior described by Smith (34) as a 'motor aura' usually occurs, typically consisting of brief quick runs, jerky sidling or backward steps, or pivoting movements of the head and body. After a latency ranging between one second and one minute, the active phase begins with a running period, sometimes of gradually accelerating speed, sometimes of a very explosive nature, and in any instance finally attaining an unusually high velocity. The running almost always seems undirected with respect to obstacles. This phase may be interrupted by one or more quiet periods, may terminate suddenly in what resembles coma, or may continue with diminishing vigor until exhaustion. Most characteristically, it leads directly (after an average of 15 to 20 seconds) into the convulsion proper. This may be predominantly of tonic extension, or incoordinated clonic beating of the limbs, or may combine both tonic and clonic aspects. During the tonic-clonic phase (average total duration 30 to 40 seconds), the following are sometimes observed: vocalization, exophthalmus, excessive salivation, cyanosis, ejaculation, diminished respiration, incontinence, dilated pupils, pilo-erection, retraction of testes, relaxation of the vaginal opening, and peripheral vasodilation, with death intervening in a very few cases. The rat may continue to exhibit clonic jerks (e.g., spasmodic jumping) with decreasing frequency as long as stimulation continues, or may lapse into the phase usually described as comatose. Most animals are very unresponsive to stimulation during this passive phase, with reflexes depressed to varying degrees. Spontaneous activity is virtually abolished, and the animal is non-resistant to handling. This condition may last for longer than 15 minutes although gradual recovery of spontaneous activity usually begins within two or three minutes.

A comprehensive review (3) of sound-precipitated convulsions has recently summarized up to 1954 our knowledge of this phenomenon, and clarifies our understanding of the multiplicity of factors which have been found to influence these seizures. The precise relevance of the different features investigated is very well evaluated in the review.

Rather late in these investigations it became clear that the incidence of these seizures in laboratory stocks was hereditarily determined. This is true for rats, mice, and Peromyscus, and Frings (16) has been able to breed strains of mice which show different patterns of seizure risk and death risk. In rats, dietary deficiencies play a part in determining the incidence. Vitamin B1 and B6 deficiencies are especially important. Magnesium deficiency is likewise effective in guinea pigs. Bevan (3) aptly comments on the relation of dietary and physiological factors to the genetic determinants when he notes that "the variability in the F_1 indicates susceptibility is a characteristic that fluctuates within a wide range about the average convulsive risk, non-genetic factors perhaps determining position within this range."

Some drugs such as nicotine, and the chlorinated hydrocarbon insecticides, facilitate audiogenic seizures in mice and/or rats, but this information has no direct bearing on the biological status of the behavior pattern. It is only relevant to an understanding of the pharmacological mechanism whereby this action is brought about and at present is insufficient in amount even for this purpose.

Otitis media has been found to be a factor in the occurrence of audiogenic seizures in rats, and has been seized on by some who claim it to be the sole cause of the incidence; but Frings and Frings (15) have demonstrated that this is not so for mice and the claims of the original report by Patton (30) do not warrant this all-embracing interpretation.

These seizures were for a long time considered to be a response to a specific type of sound, such as that emitted by jingling keys or a Galton whistle, but in the experience of the author the essential component is "transient sound" (or a rapidly varying frequency), the effect of which for rats is maximal against a background of 7 KC.

There is no doubt that the frequency of the appearance of these seizures in an animal is dependent upon the intensity of the sound, when this is present, varied either by the intensity at source or by moving the animal to different distances from the source, and on the duration of the stimulus.

On the other hand, the audiogenic type behavior has appeared in situations in which the sound components have not been noticed by the operator. Some chemical smells and smoke induce "audiogenic" type seizures in Peromyscus.

Although much dispute arose over the findings of Maier and Glaser, in 1938, that animals in a motivational conflict showed a greater incidence of seizures, it is now clear that conflict, or inability to choose between two responses, does increase the incidence of these seizures in rats.

Have we any clues to suggest an adaptive role for this type of behavior? It should first of all be noted from the description given by Munn, that this response is only one of many possible responses, all of which follow an initial startle reaction. Griffiths (20) has shown that provided the auditory stimulus is applied in an enclosure with an escape hole, rats run for shelter and do not develop the seizure. This suggests that the initial response to the sound is escape in flight to some form of refuge. Chance (9) provided several hyperexcitable Peromyscus with a plasticine hut as a refuge, and showed that not only did the mouse fail to develop a seizure if it entered the hut, but that if it subsequently came out while the stimulus was proceeding it did not develop a seizure. Further, if the animal had once entered the hut during the stimulus and it was then presented with the stimulus on repeated subsequent occasions without the hut present, the Peromyscus failed to convulse provided the interval between the repetitions was less than 30 hours. Moreover, it was shown that the hut need not possess a roof to be effective in this way. Contact with the walls of the hut was sufficient to suppress a convulsion. Thus, it is clear that convulsive behavior is an alternative to appetetive flight which has failed to establish contact with the walls of a retreat within a suitably short space of time. It can, therefore, be classified as a "displacement activity," in the sense defined by Bastock, Morris, and Moyniham (2).

Moyniham (27) found that gulls returning to their nests show displacement nest building in proportion to the number of eggs removed from the nest in their absence. Clearly, this is a displacement activity released by the frustration of the brooding drive which is normally and continuously satisfied by contact of the underside of the gull with the eggs. The convulsive behavior of Peromyscus is analogous to the nest building of the gull and is thus a displacement activity due to the frustration of the appetetive flight drive. Originating in this way it may, however, have now become a fixed alternative consummatory act within the hierarchy of activities to which the flight drive can give expression. Many such activities can be shown to have adaptive significance, often in subserving quite different drives; so it is clear that we have to define its function.

It must now be noted that the description of the hyperexcited behavior (4) culminated in a tonic or clonic, or "bouncing" convulsion in Peromyscus and that if the intensity of the sound was submaximal, either of the two latter types predominated. When this was so, the pattern of the behavior was much more constant and its features more clearly discernible. Thus, during the first phase rapid running alternated with crouching (5 to 15 seconds). During this phase, if the stimulus was stopped, the Peromyscus relaxed. This was not so in subsequent phases as then the behavior sequence would continue uninterrupted through the following stages. First, exceptionally fast running would replace the normal flight, objects would not be avoided and the Peromyscus would eventually fall over for a short bout (1 to 3 seconds) of clonic movements or pass directly into the next phase of saltation when it leaped about opening and closing its jaws and would attack any object and even the walls of the cage, digging its teeth in and holding on. This phase over, catalepsy ensued. The whole episode lasted no more than three-quarters to one minute.

Peromyscus lives in sparsely wooded country in the temperate region of America and makes its nest in dried leaves under the protection of a thicket. Thus, it is a surface living rodent more wont to frequent open spaces than is a wild mouse living, as it does, in a burrow. It is, thus, likely to be exposed to predators in the open where, sometimes, it

may find itself without readily available shelter and in relatively unfamiliar surroundings. In these circumstances a stereotyped flight reaction may have survival value, especially if, as in this instance, an original rapid run is followed by automatic aggression and finally by catalepsy. This final gesture denies to the predator the essential sign stimulus for an attack by eliminating all movement.

While, therefore, we have no direct proof of its adaptive role, because we know that it is closely related to, if not a type of escape behavior in rats and Peromyscus, we can logically deduce its survival value.

It is important to realize that ethological behavior studies of animals have revealed types of behavior like "spastic paralysis" found in the female toad at mating time, (31) comparable to spasticity regarded as pathological in man, but in the female toad this behavior is part of successful mating. Thus, it may well be that most tetrapods possess coordination units responsible for certain easily recognizable generalized forms of behavior such as clonic running, spasticity and catalepsy. These will be manifest in a pathological way only when they are not part of adaptive behavior, but appear without functional significance.

It could be argued that the experiments on rats have shown that the behavior includes many features for which no adaptive function can be deduced, either for their separate appearance or as part of an escape reaction, and this is an important point for it should be remembered that in Peromyscus the clear-cut sequence occurs only if the stimulus is submaximal. This means that it is a release phenomenon, dependent for its manifestation on the progressive withdrawal of inhibition from all brain stem centers, but that if the degree of withdrawal from internal inhibition is submaximal, the behavior instead of being haphazard is coordinated by the appearance of a definite sequence of acts. This sequence, when imposed on the automatic behavior, comprises a stereotyped escape reaction. If it is maximal, the removal of inhibition produces a state comparable to decerebrate rigidity. Between the two extremes of stimulation, intermediate levels of release may well call forth a variety of miscellaneous acts, as different parts of the brain stem become involved. Moreover, the degree to which integration into clear sequences of acts is provided by the inheritance, will depend upon the extent to which the circumstances of the animal's life have, in the past, placed a selective value on the acquisition of a "stereotyped escape reaction." By virtue of the habits of rats and mice we may deduce that the circumstances are less likely to subject the population to as much selection as is likely for a population of Peromyscus in which the pattern shows a more distinct form.

Sound-induced seizures in rabbits

Antonitis *et al.* (1), report that the first rabbits to show this type of "seizure" developed the "fits" during "disturbing environmental circumstances." Subsequently, their descendants were subjected to the sound of a bell ten seconds after placing in a box 30″ square. The behavior took the following form:

First, a "startle reaction," when the rabbit was either motionless or moved about restlessly, developed 6 to 8 seconds after the start of the sound.

Secondly, about 20 seconds later, a wild running phase interspersed with wild leaps and squealing followed, and lasted 1 to 16 seconds.

Thirdly, a clonic/tonic phase in which the balance is lost and opisthotonus may develop in the extreme tonic phase.

Finally, a relaxed recovery period.

Day-to-day and seasonal variation in frequency was noted.

This description is sufficiently unambiguous for us to be able to recognize the essential similarity of the behavior to that of the "stereotyped escape reaction" in rodents. The additional element of squealing may be present because the behavior of young rabbits in the presence of a stoat has incorporated this element in a release complex, evoking kicking attacks by adults, and appearing in the adult as a spurious phenomenon, or it may still have adaptive significance for adult animals. However, it should be clear that to confine rabbits in a

space 30″ square is hardly to allow them sufficient room to display their adaptive repertoire, and that a full investigation of the response to sound in a number of different types of environment, offering different degrees and varieties of opportunity for escape, should be investigated in order to clarify the ethological status of this excitable behavior. The authors of the paper recognize the similarity of this behavior to audiogenic fits in the mouse, but also make it clear that in common with the existing tradition of this type of investigation their interest in the behavior form is only secondary, as they suggest "that the similarity to epilepsy in man may help pharmacological testing of (anti-epileptic) drugs." This may well be, but of much greater significance and relevance at this stage of our knowledge is the fact that they have shown this type of behavior in yet another class of the vertebrates, the Lagomorpha.

Canine hysteria

Canine hysteria or "fright disease" as it is termed in America, has been described by Parry (29):

Clinical Signs of the Experimental Disease.

The disorder is described in seven distinct stages:

A. Pre-hysterical State of Hysteria-proneness.
   I. Prodromal Anxiety Stage.
B. Hysterical State of (a) Psychomotor Seizures.
   II. Barking Seizures.
   III. Running Seizures (often combined as Running Barking "fits") and (b) Grand-Mal Epilepsy.
   IV. Epileptiform Convulsions.
C. Post-hysterical State of Recovery.
   V. Automatic Behavior.
   VI. Immediate Recovery.
   VII. Residual Defects.

The principal clinical feature of the syndrome is the violent episodes of motor activity, combined with disturbances of consciousness. There may be simple running seizures in which the animal runs about violently as in flight, often colliding with objects. The animal is not fully cognizant of its surroundings and apparently has no recollection afterwards of events, during the seizures, in which it may travel many miles. There is a tendency to be aggressive, even toward well-known human friends, if the dogs are approached roughly, but the aggressive attitude is one of defense rather than of attack, and it is remarkable how in many cases a seizure can be cut short by sympathetic human handling. The dog sometimes barks in an unusual high-pitched manner during the running seizures, and such barking may occur without running attacks. These constitute the familiar running barking fits and may last from a minute up to several hours. In addition generalized grand-mal epileptiform convulsions occur with tonic and clonic phases and complete loss of consciousness and of motor control of sphincters; these last 30 to 60 seconds and often leave the dog exhausted. Both types of motor episodes may occur together, one following the other closely; the epileptiform convulsion usually precedes the running seizures, but on several occasions the convulsion has immediately followed a running seizure.

Before the acute motor episodes the dog often exhibits well-marked anxiety and apprehension, which may persist for days and weeks, without any motor episodes being observed or suspected. This prodromal anxiety is by no means a constant feature.

After a motor episode a dog may appear almost normal at once, with apparent dissolution of the prodromal anxiety state; on other occasions the anxiety is enhanced; in others again a short period of automatic behavior develops which may easily be overlooked or be confused with the signs of prodromal anxiety, and after protracted and frequent motor episodes various degrees of exhaustion, coma, and ataxia occur. In a small proportion of cases residual neurological defects are found, especially impairment of vision. However, in general the disorder is completely reversible.

Many of the same elements are present in the dog's behavior during "hysterical fits" as in the rodent's during audiogenic seizures, but they appear in a variable sequence, like in rats, rather than constituting a "stereotyped flight reaction" as in Peromyscus. For example, the hyperactive flight may follow rather than precede a clonic or tonic phase. Barking is a new element and is similar to the high pitched squealing of young rabbits while crouching as if frozen to the ground at the approach of a stoat. In the rabbit, this squealing has a social significance since adult rabbits in the vicinity have been observed to respond by leaping over the squealing rabbit while it is being attacked. During the leap they lash out with their hind legs and can put stoats to flight in this way.

The stimulus situation for dog hysteria is clearly one of startle and confusion and is enhanced by nonspecific emotional stimuli.

It is possible, therefore, that in the wild this barking may have a similar social significance in obtaining protection by calling together other dogs.

As with audiogenic seizures, a multiplicity of factors have been found to influence its occurrence. Many types of dog exhibit this type of behavior during certain periods in the life history, e.g., in suckling bitches and during the teething period in 4 to 6 month old puppies. Related to these factors are the more obviously emotional stimuli such as those described by Parry:

> Neuropsychological hysteria-precipitating factors are in the main exteroceptive stimuli mediated by auditory and visual receptors, though tactile ones may play a role. It is well-known from clinical experience that a loud or sudden noise, such as the back-firing of a car engine, or an apparently harmless situation which has acquired an emotional association for the dog, such as the scene of a previous fight or an unexpected rebuff from a human friend may precipitate episodes. That such factors will precipitate episodes in experimental hysteria we have had repeated experience, and even the illumination of the room can be the critical factor.

It is significant that inquiries among veterinary surgeons attached to kennels reveal that this is a prominent "abnormal" element in the behavior of highly inbred greyhounds where the social excitement is a potent precipitating factor.

A variety of diet factors, among which wheat glutein is important, also play their part in the different dietary routines of many domesticated dogs. Inbreeding will affect the variability by affecting the biochemical potential. Sang has reported that inbred strains of Drosophila are less biochemically efficient in terms of protein utilization than are outcrosses. On a given diet, therefore, the possibility of a metabolite reaching this level will be a function of the genetic constitution.

The possibility of the secondary dietary causation of "fright disease" being due to this phenomenon must not be ruled out.

Among the purely infectious causes are neurotropic viruses and parasitic infections.

Orgastic convulsions

"Extensor tension with violent convulsion" is reported by Kinsey *et al.* (22) for "about one sixth of pre-adolescent boys and a small percentage of adult males." "... human females and males may react in the same way." Writhing convulsive movements have been observed in female cats, tigers, and lions following copulation. Both sexes of the rabbit are thrown to their feet by a convulsive jerk accompanied by a shriek. At present, insufficient knowledge exists for us to be able to ascribe any ethological significance to postcopulatory convulsions, but it is possible that they are tolerated shedding activities, providing a means for reducing the aggressive and flight drives at orgasm, which are together aroused in association during courtship. The arousal of flight and aggressive drives during courtship has been demonstrated for non-mammalian vertebrates by Morris (26).

Inhibition and convulsions

The two types of adaptive seizure in rodents present the following picture.

We can show that the falling posture subserves a function and also that it is triggered off by the absence of support—probably the absence of tonic inhibitory stimuli from the otolith of the utriculus (7).

There are, therefore, two instances of adaptive convulsions, and both are in common triggered off in circumstances which provide no external clues with which to correlate movement. Both are appropriate to situations in which vision provides no information for correlated action. In each instance, coordination is provided from within.

It is now worth noting in which ways the convulsions can be inhibited. Inhibitory stimuli can prevent audiogenic seizures in Peromyscus (9), both directly as a result of the consummatory act bringing the Peromyscus into contact with the walls of the hut, and indirectly through learning when the repeated stimuli fail to produce a seizure in the absence of the hut. Inhibitory stimuli of contact with the floor terminates the convulsion in the falling mouse and in this instance the absence of the same inhibitory stimulus may contribute to the start of the convulsion. In the falling mouse, moreover, the midbrain does not fully participate in

the characteristic biochemical changes (glycogen increment) unless the righting reaction is superimposed on a fall. Thus, during a fall there is a suggestion that partial inhibition is maintained. These instances make it clear that convulsive discharge is not outside the graded control exerted by various forms of inhibitory brain function, and must, therefore, be considered an integral part of normal nervous function, at least in rodents.

Nor should it be forgotten that the external clues present before the behavior is "triggered off" are important, as these can either suppress the behavior completely (learned suppression) or increase the likelihood of its occurrence, if a conflict of clues is present.

The results of Maier and Glaser's (24) experiments are more illuminating than they have been taken to be, for the controversy over the role of conflict has left the original finding confirmed; namely, that a conflict of clues increases the incidence of seizures in rats, other factors being constant. This feature is one aspect of a more general situation, i.e., an absence of directional information.

Now that audiogenic seizures are seen to be an alternative stereotyped escape reaction to directional escape by flight to a refuge, we are able to see that a partial or complete absence of appropriate clues is a constant feature of all the experimental conditions.

Since complete confinement is a simple way of keeping the animal close to the source of sound, it is also possible to see that if the behavior of the animal in response to the start of the sound does not lead to a rapid reduction in intensity, this itself would, in the wild, indicate that the flight was proving unsuccessful and, therefore, prolongation of the stimulus may, other factors being constant, be sufficient to trigger off the stereotyped escape reaction. We are able to see that the way to assess the significance of individual contributory factors in the initiation of audiogenic type seizures, is the appropriateness of each element to the specific situation in which escape behavior is engendered.

Incidence within a population

The incidence of seizure susceptible individuals within a population varies from 100% for the falling posture for all strains of mice, and for specially selected strains of mice susceptible to audiogenic seizures at one end, to mouse strains, strains of Peromyscus and many strains of rats showing much smaller percentages. A number of animals in any colony of mice show the falling posture when held up by the tail, and by selection for this feature Hagdoorn produced a strain of H.C. mice which possessed a high proportion of such mice. Thus, there is an exact parallel between this instance of the falling posture in mice, which in some individuals is triggered off at a lower threshold of the stimulus when held up by the tail, genetically determined, and the audiogenic convulsions found in Peromyscus, one generation removed from the wild population, being also hereditarily determined and capable by selection of producing a strain with a high incidence of susceptible animals. Breeding experiments, both with Peromyscus, mice and rats, suggest that it is a polygenic effect. Presumably, therefore, the relatively unstructured features of the behavior in rats is due to lack of specific selective pressure in the more immediate past. It is certainly interesting to note that the hereditary constitution of mice as well as Peromyscus is capable of restricting the incidence to a certain age range; in Peromyscus up to maturity, and in the Frings' strain of mice between 17 to 25 days old.

Selection of the H.C. strain from the normal stocks produced individuals showing a form of behavior present in 100% of the population, but more sensitive to the precipitating stimuli, i.e., smaller pull of gravity (G). It is thus very possible that the incidence is not only controlled by factors influencing phenotypic emergence, but also by genes influencing relative excitability. This is to be expected in a feature of behavior which appears only in association with excitement. The sizable incidence of orgastic convulsions in man and other animals may, on the other hand, be no more than an expression of the absence of selec-

tion against such erratic behavior which, because of the high level of excitability during copulation, may engender conditions favorable to convulsive discharge.

Polyethism and the classification of convulsions

It is clear that with the exception of the falling posture, convulsive behavior is confined to a proportion of the population which is termed "susceptible." Nevertheless, it is clear that in the case of the "stereotyped escape reaction" in mice at least, this restriction is a genetic characteristic and can be broadened by appropriate selection to include practically the whole population. Its presence in a few immature individuals of the wild population, therefore, as in Peromyscus, is an indication that the selective advantage is best served by the presence in the population of both animals which possess this and animals which do not. The adult and/or adolescent population of a rodent, such as Peromyscus, exists in two types which though possessing no obvious anatomical difference, such as the difference between adult forms of ants, are nevertheless different in their repertoire of behavior. On analogy with the phenomenon of polymorphism in ants, which is encountered in other invertebrate phylla, we may term the possession of different behavior repertoires in different members of the same species at the same stage of growth, polyethism. In the "stereotyped escape reaction" we have the first example of such differences between individuals in mammals, other than that dependent upon sexual differentiation.

The stereotyped escape reaction, however, achieves this status in the behavior of Peromyscus by virtue of occupying an intermediate position between the type of behavior represented by the falling posture on the one hand, and orgastic convulsions on the other. At one extreme, as in orgastic convulsions, the convulsive behavior is normal, polyethic, unstructured, and without adaptive significance; it is, in the absence of evidence to the contrary, a tolerated aberration. At the other extreme, the behavior is normal, monoethic, structured, and adaptive. Whenever a structure other than tonus or clonus is present, therefore, it is because the "convulsive discharge" plays a role in adaptive behavior. Similarly, the degree to which an adaptive convulsion is held in the population may be expected to depend on the degree of selective advantage that the particular type of behavior imparts to the animals in a particular environment. This aspect requires systematic study. Nevertheless, since unstructured convulsions have so far been found to be always polyethic, we must consider the role of other factors in the genesis of convulsions. In all strains of rodents many other factors influence the day-to-day or seasonal fluctuation in susceptibility and an important variable is the emotional reactions of the animal. Since convulsions appear as part of excitable behavior we may assume that the polyethic manifestation of convulsions may represent the fact that some animals of a population lie at one end of a spectrum of excitability, which precipitates convulsions in them and is not sufficiently high to do so in others. That this extreme excitability is contributed to by hypothalamic excitability is very likely.

We can now reintroduce the concept of normal in this connection, by pointing out that populations of mammals are made up of individuals distributed over a range of excitability which is a characteristic of mammals as a class, and that at one end of the excitability range convulsions appear as a normal manifestation of high excitability. Reference to Fig. 1 will clarify the concept. Figure 1 shows also that normal convulsions produced by intrinsic stimulation may exhibit a more or less definite structure. On the one hand, there are the spasmodic movements of orgastic convulsions, and on the other, the complex posture of the falling convulsion. In between lie the various elements of release behavior, some of which form the stereotyped escape reaction possessed by some members of a Peromyscus population. It should be noted as well that some mice show this escape reaction in a form as rigid as that found in Peromyscus, while on the other hand some do not, and also that in rats the elements of this reaction appear together or separately, combined

	ETHIC: Mono	ETHIC: Poly
Structured	Falling posture of mice (and other small mammals?) I.S.	S.E.R. Peromyscus (S.E.R. in rabbit and dog?) I.S.
Intermediate	Audiogenic seizures in selected strains of mice. [Frings] I.S.	Audiogenic seizures in mice, rats and guinea-pigs. I.S.
Unstructured	C.D. 100 of drugs and electrical stimulation. E.S.	Orgastic convulsions in man, felines, (and rabbits?) I.S.

↑ Increasing Functional Role

S.E.R. = Stereotyped escape reaction.
( ) = Doubtfully classified.
I.S. = Produced by intrinsic stimulation.
E.S. = Produced by extrinsic stimulation.

FIG. 1. Classification of Non-pathological Convulsions.

with other elements such as cries, evacuation of feces and urine etc. One may assume then that selection has bound together a number of motor elements out of a wide variety that are released by generalized disinhibition of brain stem centers. Those that serve a function in escape have been bound together into a motor coordination unit which produces the adaptive sequence of the stereotyped escape reaction.

The ethological status of convulsions in man

During recent years the tendency has been to enlarge the group of pathological phenomena classified as epileptic to include the finer manifestations of "petit mal" and the behavior disorders often associated with "grand mal," such as outbursts of aggression, catalepsy etc. The basis for this broadening of the definition appears to be the sudden obtrusion of these elements into an otherwise smooth sequence of related acts. The investigation of the chemical changes in the nervous tissue of the mouse brain leaves no doubt that there exists a unique cellular activity underlying convulsive muscular activity in this animal, whether or not the nervous discharge arising from this unique activity gives rise to adaptive behavior or manifests itself in clonic/tonic movement comparable to "grand mal" epilepsy in man. It would seem likely that the superficial resemblance between clonic and tonic convulsions in mice and men actually reveals an underlying similarity of neuronal state. Since it would be difficult to equate the nervous discharge in catalepsy with this type of discharge without envisaging prolonged states of convulsive activity comparable to status epilepticus—and on present evidence this, though possible, is unlikely—we would be wise, in comparing the evidence on the status of convulsions in animals with that in man, to restrict our attention to clonic/tonic manifestations such as appear in "grand mal" type convulsions.

If this restriction in meaning is accepted, as a basis for argument, then it is clear that man exhibits two of the three categories of convulsions found in rodents, e.g., polyethic normal and pathological convulsions, but no adaptive forms, so far as our present awareness allows. Now, if on the other hand we widen the definition of convulsions in man to include the classical "petit mal"—loss of consciousness and muscular tone—then we should take note of the work of Lennox, Gibbs, and Gibbs (23), who have suggested that an essentially neural mechanism exists in certain persons with hyperactive sinus reflexes to bring about unconsciousness, resembling in this respect cataplexy and reflex epilepsy, without the intervention of a general cerebral anoxia. This reflex operates if

the O_2 tension of the blood flowing to the head falls below 24%; a level much above that which interferes with muscular metabolism.

This reflex is, it is suggested, adaptive in that by prostrating the body it enlarges the circulation to the brain before the level of blood flow in the circumstances of reduced oxygen tension reaches a level so low as to cause damage to the brain. This adaptive reflex is protective and, if it is a fact, is evidence of a polyethic adaptive convulsion in man. The justification for this latter view must rest, however, on further work in the future in attempting to identify the unique neurochemical state underlying grand mal, which should not be impossible during the neurosurgery of focal epilepsy, and ultimately to the discovery of some feature which will enable such a state to be recognized in the unoperated individual.

Conclusion

The relation of animal experiments to human pathology is very much in need of theoretical assessment. The purpose of this review is by no means exhaustive, however; it is concerned with assessing the implications of a few recently acquired facts about convulsions in mice which have made possible a biological interpretation of convulsions as they appear in a number of different types of animal and under a variety of conditions. The evidence is sparse but theoretical exploration is vindicated if fresh means of obtaining evidence are suggested.

The evidence leaves little doubt that convulsive discharge can play a part in adaptive behavior in the mouse. This means that it must be regarded as one of the inherent modalities of behavior like, for example, emotional excitement, which are pathological only insofar as they are not part of an integrated function, or are the result of identifiable organic pathology.

It is also suggested that because of the genetic variations within a population the parts of a behavior pattern which may be fully integrated in some individuals are found only partially so in others.

This proposition rests not only on an attempt to define the adaptive function of the stereotyped escape reaction as shown in a strain of Peromyscus under audiogenic stimulation, but also on the logical assumption, widely accepted in biology, that if a phenotypic element appears before maturity, in wild populations, it must be held there by selection and, thus, will be found to have an adaptive significance. The existence of less well-structured audiogenic convulsions in mice and other animals may thus represent a more recent evolutionary loss of adaptive function for an element deeply embedded in the genotype. Such elements are recognized as vestigial organs in anatomical studies; we are probably witnessing in the study of audiogenic seizures, the manifestations in behavior of such a structure within the brain.

It is clear that emotional excitement is not a part of the falling posture in mice; this form of central excitability is not an essential precursor of convulsions. Nevertheless, the fact that in both conditions which initiate the falling posture and the stereotyped escape reaction, external integrating stimuli are missing, does suggest inhibitory influences are low when a convulsion arises. Thus, a search for the causes of convulsions should seek rather in disparity between inhibition and excitation, rather than any absolute measure of either. It is clear, however, that the disinhibition can arise in some mammals, and possibly in man, from mechanisms forming a part of the integrative functions of the brain as well as from the operation of purely physiological or pathological causes.

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CHILDREN IN A SOCIETY UNDER STRESS

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Much is said about stresses which a society undergoes when its way of life is drastically changed or threatened. How are these stresses reflected in the individual? Here the authors undertake to examine the perceptions of situations by children of the Northern Cheyenne Indians. They apply the techniques of the Thematic Apperception Test appropriately modified to fit the experiences of those children.

ADEQUATE knowledge of the significance of cultural change for human development is yet to be obtained. It is not possible at present to show validated cause and effect sequences between the culture and the individual personality and behavior. Therefore, in this study we propose to answer only two questions: 1. what are some essential characteristics, in outline, of one changing society which might significantly influence human behavior and personality; and, 2. what are the main characteristics of perceptions of some of the developing individuals in such a society? From the viewpoint both of the anthropologist and the psychologist, cultural and societal disintegration theoretically will affect the individual's perception of his world and his ability to cope with it. However, we shall not endeavor to establish an unvarying relation between the culture and the characteristics of individuals which would prompt us to maintain that because of a specific characteristic of the culture, a specific perceptual response results. We do attempt to examine in this study the possibility of the reflection of a disintegrating society in the perceptions of its developing members. We try to avoid in this study any intercultural comparisons between the culture under study and that of our own. That is, our aim is neither to compare the cultural outline of the society in this study nor the personality characteristics of its members with our own society. Our sole aim is to examine this disintegrating society for evidence of the disintegration being reflected in its individuals.

We are aware of the tremendous complexity of the problem both from a theoretical and practical viewpoint. We do not assume here that the problem involves only a responding organism in an environment with certain forces playing upon it—we recognize the theoretical difficulty in adequately defining the terms "culture" and "personality" and the practical difficulties in distinguishing between the realities comprehended under the terms. We seek only to separate for our problem certain stimuli offered by the culture and certain perceptual responses made by immature individuals who are a part of the society under consideration.

The society under stress which we are studying is that of the Northern Cheyenne Indians. With respect to individual behavioral and personality characteristics, the Northern Cheyenne have not been studied as have the Navaho, Hopi, Sioux, Papago, and Zuni. We refer here to such thorough and significant studies as those by Henry, Macgregor, Thompson, Leighton, and others (8, 9, 10, 11, 12, 15). No recent whole-culture study of the Northern Cheyenne has been made, aside from that of Anderson which is in progress and from which the data here presented have been taken.

We have undertaken a simple division of labor, centering contributions around two concepts, *culture* and *personality*. In general, by culture we mean to include the tools and techniques for making a living, patterned

social relationships, and attitudes and beliefs toward the natural and supernatural worlds, all of which comprise a system—persistent, albeit changing—into which the individual is born and in which he lives. In general, by personality we mean the system of characteristic responses made by individuals as a result of physiological capacities and factors, and, forces from the environment culturally derived—the expectations and demands of other people and the influences of the natural world. Included in this definition are organization and integration of responses into a totality of organismic response. There is no real conflict: the anthropologist's emphasis is on the supra-personal quality or aspect of the group's behavior, and the psychologist's interest lies primarily in the individual and the basis for his behavior. We believe that in the course of human development the physiological needs become so entwined and influenced by cultural factors that it is perhaps impossible to find a stimulus-response sequence which is uninfluenced by cultural conditioning.

It seems possible to assume that an individual's perceptions tend to have certain qualitative characteristics as a result of the fact that maturation is taking place in a given cultural milieu and that perceptual similarities in different individuals of the same cultural segment may be discovered. Again, as with culture and personality, perception is a difficult term to define. Broadly, it is awareness of stimuli, or energy change. Gleaned from the broad meaning of the term there are certain concepts which are particularly appropriate to this study. There are, of course, many excellent treatments of perception and it is necessary for us to mention only those concepts which are appropriate for this study and which have a particular bearing on our task (6).

First, perception involves *selection* of stimuli to which to respond. Selection comes as a result of need or drive and conditioning. Conditioning is begun early in development and continues throughout life. Selection thus is based in part upon experience and in part upon need. It is possible then for need and experience (or conditioning) to have considerable influence in determining stimuli to which a person will characteristically respond. We shall undertake to study the selection of stimuli presented to the subjects in this study and also the selection of stimuli which the subjects characteristically add from their experience.

A second point is that perception ultimately involves the *whole organism*. The process of perception begins in the receptors. However, it should be remembered that a vast chain of internal events takes place as a result of a stimulus activating a receptor. Some of these internal events involve the stimulation of the emotion machinery. Stimuli with certain meanings come to evoke certain emotional responses.

A third point which we believe to be important is that perception involves an assignment of *meaning*. Of course, this meaning is a result of conditioning and experience. We tend to expect certain meanings from stimuli we encounter. That is, we approach a new situation with the belief that it will contain certain meanings and opportunities favorable for need fulfillment or we may characteristically expect hostility and deprivation—that the new situation will hold nothing for us.

This search to determine the characteristics of specific perceptions of certain cultural groups may in the future lead to a better understanding of the significance of the many cultural factors influencing human development. Our first task, then, is to sketch the cultural background of our subjects and to epitomize it with respect to characteristics which may aid us in understanding the perceptual responses in that culture. Our second task is to characterize certain of the perceptual responses as we found them.

DATA AND SUBJECTS FOR THE STUDY

The data concerning the culture were gathered by Anderson from Cheyenne living on the Tongue River Reservation in Montana. The anthropologist's techniques were the common ones of participant-observer and interviewer. A thematic apperception test for the Cheyenne was developed by Alexander in order to secure the psychological data. In this study responses were

used from seven children, five girls and two boys, with an age range from ten to fourteen years.

THE NORTHERN CHEYENNE: CULTURAL BACKGROUND

General characterization

The Northern Cheyenne are a cultural enclave economically and socially marginal to the world of the white ranchers, farmers, and townsmen of south-eastern Montana. They live behind fences and administrative barriers on the 445,000 acre reservation of range and forest land on which they were placed near the end of the last century, and which represents part of the gamelands they last occupied when they were a typical Plains bison-hunting tribe (7).

In the limited space we have here, we wish to emphasize three sets of facts which the anthropologist would suggest to be relevant to interpretation of our psychological data. The first is the social separation of the reservation community as a whole from the surrounding white community, a complicated product of different culture, language, history, and administrative situation. There is intercourse and interdependence, but there is no mistaking the separate worlds. The second is the low economic state of the people, with its attendant potentiality for feelings of personal insecurity and anxiety, both directly from fear of hunger, and indirectly from wider social strains which this situation may engender. The third is that the Cheyenne, although in one respect poor rural Americans, are unlike "ordinary" poor rural Americans in that the heritage of aboriginal culture remains strong, and that in some ways it mitigates and in other ways exacerbates the results of poverty. New institutions are in the process of growing from old ones, old and new institutions serving the same cultural purposes co-exist, and at times are in conflict, and institutions without analogs in the old culture have but recently been established.

The families of children whose responses to our projective test provide the body of this report live in or near the Birney "camp," a population cluster whose principal reason for being is a Government day school with an enrollment of 40 to 50 pupils in eight grades.

Living conditions: Housing, diet, health

In a context of white communities, Birney would be labeled a rural slum. Its approximately fifty households live in small one- and two-room ill-furnished log cabins with earthen roofs, lighted by kerosene lamps, and without indoor plumbing or water. Families of two to ten persons may live in a single room. Water is carried from the Tongue River or from wells; toilets are outdoor privies. Cooking is done on wood-burning cast-iron stoves, which also provide heat. Tent-living is common during the summer.

"The nutritional status of the Indian is generally much below the optimum," according to a statement of the Montana state board of health in 1952 (13, p. 4). Menus for even the relatively less poor families show a high proportion of fried and canned foods and white breads, and few or no fresh vegetables, fruit, and milk. There is no evidence, however, that bulk intake is inadequate.

The health situation has aroused concern repeatedly. It is no longer true that "This reservation is literally a plague spot," as an investigator for a Senate committee reported from the field in 1926 (16, p. 12841). However, the present death rate among the Cheyenne from tuberculosis is very much greater than that of the state as a whole.

Also the rate of infant mortality is approximately six times greater for the Cheyenne than for the state of Montana (17). One would be justified in saying that the Cheyenne are exposed to a health hazard considerably greater than is the average white Montanan.

Making a living

In a limited space, it is impracticable to document the economic situation in detail. However, descriptive and statistical corroboration (4, 17) is available for the following general statements. The Cheyenne live in a cattle and wheat region, but few are breeders and farmers. Few live on their allotted land; for the most, the land is rented for them to non-Indians, for which

they derive lease checks of roughly $60 a year per allottee. Median family income in 1950 was $963, with the lowest 10 per cent averaging $191. Eighty per cent of the Cheyenne in 1950 received some form of welfare payments or relief, which represented a quarter of the community's income. Thirty per cent came from labor, most of it of the casual, seasonal type at ranches and in the beet fields. From present prospects, few will pull themselves out of the unskilled worker class in the near future, inasmuch as many discontinue school between the fourth and eighth grades, and out of 70 beginners, only 11 reach senior high school.

Given this low income and job instability, the question may be raised, how are these Cheyenne families able to live? The answer is, by exercise of an aboriginal practice, carried out through the old network of kinsmen and neighbors—by sharing. When an able-bodied man has a job at a ranch, relatives may move in with him and find a place at his table. The smallest lease check and welfare payment may filter through a widening circle of in-laws and neighbors. A slaughtered beef or bag of game is parcelled throughout the neighborhood. It will be recognized by those acquainted with American Indian life that this rule of sharing and hospitality is a heritage of the past.

Maintenance of the obligation of generosity to those in need and hospitality to kinsmen and friends is jealously fostered today. Violators of this vital social convention are held up to scorn, and it has been said that more than one Cheyenne has found it meet to leave the reservation if he would keep all of his wage and increase for himself. "It is evident that in this area of socio-cultural relations two sets of attitudes engendered by two cultural traditions are in conflict. A Cheyenne who would progress economically must amass some capital and harden his heart against the promptings of old cultural attitudes of sharing. But in so doing, he cuts himself off from his people, who labor under the old virtues" (4, p. 247).

We also see collision of values in an allied situation—inheritance. What was once shared with a wide circle of relations and the community through inheritance and gift-giving upon death of a member is now increasingly restricted to the nuclear family, a solution encouraged by white law, now that such matters are adjudicated by the state of Montana, but a source of conflict, as records of the tribal court bear witness.

Another aboriginal situation is economically and socially consequential. This is the persistence of summer visiting and migration, when families take to canvas and share the board of relatives and friends in different parts of the reservation. Occasions for communal gatherings are the aboriginal Sun Dance at this and other reservations, and numerous fairs, rodeos, and roundups. This practice has an adverse effect on the economy when it results in untended cattle and crops and neglected jobs, but it undoubtedly reinforces the network of social relations upon which sharing rests.

Factors of marriage and family life

In aboriginal days, the extended matrilocal family or "camp" was more stable than the nuclear families that composed it. Divorce was relatively easy and frequent, and children of a broken marriage remained with the mother in her father's camp. Marriage and divorce are now regulated by Montana law, but it could be reported that at least in 1949 "the old practice of marriage by the act of cohabitation and founding a new household, with consent and approval of relatives, persists to a degree that distresses the missionaries and disturbs tribal officials" (4, p. 232).

Simple matrilocality today is upset by availability of room in the wife's household, uncertain food supply, and relative dependence of elders, but in event of divorce, children still generally remain with the mother. However, economic stress may lead the second husband to balk at providing for children he did not father. And there are other divisive factors for which we do not have statistical evidence but which, according to welfare workers and missionaries, indubitably exist. It is believed that in submarginal homes with several children, the parents may separate, the mother taking the children in order to apply for aid for them which might otherwise be denied if a male

breadwinner, even though unemployed, were present in the home. And further, it is held that the expectation of relief payments for children encourages illegitimacy. Needless to say, the Indians themselves deny this.

Toward their children, the Cheyenne appear to have maintained the manner of guidance which was characteristic of them in aboriginal days. The following expression by a female informant is believed to be typical: "We never whip our kids. Once in a while they get into a fight. Their dad calls them in and talks to them and tells them what is what and what they shouldn't do. Everything is like that. 'Behave yourself,' he says. 'In company sit down and learn, never speak before company. If you want to talk, go out and play, so you won't disturb who comes in to see him.' They were always told to be gentle, and they were always pretty good" (4, p. 233).

Conflict in society at large

Not only in the economic and social aspects of family life, but also in political affairs the Cheyenne community has been shaken by emerging institutions and realignments of authority. There are two aspects of leadership and two groups of leaders in the tribe today. The first rests upon the past, represented by the conservative elders and priests, repositories of tribal tradition (5, p. 273). In the past, it was they who were the peacemakers and providers for widows and orphans. Now family quarrels, as often as not, are taken before tribal court, appointed by the tribal council. The second rests upon present economic power, and is represented by elected council members, the more prosperous farmers and cattlemen, and ambitious politicians of several degrees of acculturation. In this latter area, factionalism has arisen between "longhairs" and "progressives," and a factor in the conflict is a new religious cult, organized as the Native American Church.

The supernatural lore of the bison-hunting days, the old beliefs in the relation of man to nature, find expression today during the Sun Dance, in the healing efforts in the sweat lodges, each with its bison skull near the eastward-oriented entrance, and in the wearing of amulets. Stronger is the more recent peyote cult, which has amalgamated Christian and pagan elements. Missionary efforts have been maintained for many decades, but the churchmen of the two dominant Christian groups on the reservation claim only small cores of faithful parishioners. But, like the churchmen, the aged priests of the old faith lament contemporary indifference toward the sacred as they define it. In this area of life, too, the Cheyenne faces two ways, and the impression of the observer is that beliefs with respect to the supernatural are diffuse and disorganized. Standing as foci for the irritations and resentments to which all these conflicting institutions, loyalties, and values give rise are agency officials, the Office of Indian Affairs, and the national government.

The anthropologist would suggest that conflict and want need not be experienced directly and immediately by a child for their effects to be evident on him. The stresses and strains we have discussed are characteristics of the general socio-cultural situation. The child lives in a community which is in a condition of disequilibrium and conflict. Logically, the anthropologist would say, there might be aroused fears in the child that his own household might be broken, or the food supply might fail, or a kinsman upon whom one depends for security might with determined effort move out of the Cheyenne orbit—bodily or by assumption of new attitudes and habits. On the reservation, the phrase, spoken in Cheyenne, is frequently heard: "We are distressed."

THE INDIVIDUAL IN A SOCIETY UNDER STRESS

This is then an outline of the culture of the Northern Cheyenne. What are the significant perceptual responses of individuals in such a culture? We have undertaken to secure responses by the use of a thematic apperception technique. This technique was first used effectively in research by Henry A. Murray. Later it was adapted by William E. Henry and used in the study of people in Indian cultures as part of the Indian Education Research Project carried on jointly by the Committee on Human Development of

the University of Chicago and by the United States Office of Indian Affairs (15, p. 7). Our test was developed specifically for this study. It consists of five pictures drawn from photographs taken by Anderson among the Cheyenne. The first three were based on content and adapted from Murray's TAT (14) and the last two were developed from the Anderson photographs only.[1] The pictures are described as follows:

Picture 1. A small boy is seated in the doorway of a log cabin. His elbows rest on his knees and his chin on his hands. (This picture is similar to Murray's TAT, 13B.)

Picture 2. A woman is seated close beside a girl, but not speaking. The girl holds a bowl in her lap and looks away. (This picture is similar to 7 GF of the TAT.)

Picture 3. A woman is seated looking back over her left shoulder at a man standing behind her. The man is looking at her. (This picture is similar to 6 GF of the TAT.)

Picture 4. A man in full war dress with a bow in the left hand faces the left margin of the picture. His right arm is across his body and his chin is tilted upwards. In the background is a fence and some mountains in the distance.

Picture 5. A tipi is in the foreground with a dim figure walking toward the tipi from the background. Storm clouds are in the sky.

Administration of the test

The Cheyenne children were asked to write their responses to the test. They were given the test cards and asked to proceed from one picture to another, writing the stories on separate sheets of paper. The directions given were as follows:

Write what you think about the pictures. It will be like a story and you are to tell: (1) what is going on in the picture, (2) what went on before, and (3) what the people in the pictures are going to do next.

Examples of protocols

Two of the protocols of the children are given in order to show the method of analysis and how the data were derived for the tables 1 and 2.

Susan Sunwalking: age 12, 5th grade.

Card 1: That little girl is sitting outside. Maybe her mother die and maybe she never did eat and she got no shoes. I guess she was alone and she was so lonesome and her father had die, too. They were live out in the forest and the girl stays out in that forest. The end.

Card 2: That woman is talking to her girl Maybe that girl (is) sick and she doesn't want (to) eat. Maybe that woman is telling her to eat but she won't eat. The end.

Card 3: I guess (she) was mad and told him to get away from home. But that men (man) doesn't want to get away. And then that woman was sure got mad, and that (man) went away. The end.

Card 4: That Indian was hunting for deer or bear and then he heard some thing running and then he told his men to get that deer or bear so they (get) it, take it to him. The end.

Card 5: It was going to rain and it was blowing and that man was tie the teepee hard and it was evening and it was going to rain hard. The end.

Henry Prairiedog: age 14, 8th grade

Card 1: Once there was a log house. A little girl live in it. Her father die when she was a little baby, then her mother starts another man, then her stepfather make a log house, then her stepfather die, then she got no father, then about in two years then her mother die. Now she is sitting outside worrying about her mother and two fathers. That's all.

Card 2. About more than ten years ago one lady took her little girl out in the woods telling her a story about the fight in the West. They were fight white man. The little girl was setting on something she got a bowl in her hands, she must (be) going after something to eat, maybe berries. She is Indian girl, she got a dress make out of buckskin, and the woman got a dress make out of buckskin too. The woman got a long hair and the girl too. That's all.

Card 3: Maybe this story is about today, the woman and man got white man's clothes. The woman is looking at his husband. The husband is looking at his wife. They were saying something. The woman got a long hair and the man never did get a hair cut, both of them were in there house, the man is standing up and the woman is setting on something. That's all.

Card 4: Here's an Indian with a bowl (bow) in his hand and I don't see arrow, maybe he's got it in his back, I don't know.

Card 5: Once when Indians still live in teepees, there was a village. Many teepees, many people. There was one man that don't want to live in the village, then he told his wife we shall go away from village, then they start picking up, then they went away from the village when they just got put up there teepee they saw the rain and a lightning was coming. They let the horses go, they went in the teepee. It was rain hard. That's all.

[1] The pictures for the test were drawn by Theron Alexander, Sr.

TABLE 1

ANALYSIS OF PROTOCOL OF SUSAN SUNWALKING, AGE 12, 5TH GRADE

Analysis Categories	Cards				
	1	2	3	4	5
Stimuli presented to and used by *S*	girl	girl woman	man woman	Indian	man rain teepee
added by *S*	mother shoes forest	—	home	deer bear hunting	blowing evening
Emotional Expressions positive	—	—	—	—	—
negative	lonesome	sick not eat	mad	—	—
External Forces beneficent	—	—	—	—	—
hostile	death (father, mother) no eating no shoes alone	rejection of food and woman's persuasion	woman mad, told man to leave and he did	—	blowing hard rain hard

TABLE 2

ANALYSIS OF PROTOCOL OF HENRY PRAIRIEDOG, AGE 14, 8TH GRADE

Analysis Categories	Cards				
	1	2	3	4	5
Stimuli presented to and used by *S*	girl log house	girl woman bowl in hands buckskin dress	man woman (wife) long hair	Indian bow buckskin hills posts	Indians teepees man rain
added by *S*	mother father stepfather	woods fight in West white man berries	white man's clothes house	arrow Sitting Bull (fight or hunt deer) hair	villages many people wife horses lightning
Emotional Expressions positive	—	—	—	—	—
negative	—	—	—	—	—
External Forces beneficent	—	mother	—	—	—
hostile	death (father, mother) stepfather	fighting white man	—	fight	(deprivation) did not have horses

METHOD OF ANALYSIS

In order to examine the data in regard to the three points made above in regard to perception (selection, emotion, and meaning) tables were developed with three main categories. The first category is that of *stimuli*. Under this heading are placed the subcategories *presented to and used by S* and *added by S*. The first of the subcategories refers to the cards and the stimuli presented on them. Presenting a subject a card constitutes a situation to which the individual may respond by selecting a range of stimuli. Responses may range from ignoring the card or using only a few stimuli, to using a great number of small details contained on the card. The second subcategory refers to the stimuli which the subject calls upon from his experience to help him in dealing with the cards. These stimuli he believes to be important to him and yet are not contained in the cards.

The second category refers to the *emotional expressions*. As has already been explained perceptions are usually accompanied by some emotion. Under this category we sought to obtain data about the emotional experience and the emotions the subjects believed appropriate in dealing with the stimuli. Thus the records were studied and the emotional stimuli were placed in positive (pleasant) and negative (unpleasant) subcategories.

The third aspect of perception in our study is that of *meaning*. We undertook to discover the characteristic meaning assigned to situations as to whether the subjects believed forces in their environment are generally beneficent or hostile. Thus, there are subcategories under *external forces* to classify the views of the subjects in this regard (1, 2, 3).

RESULTS

Tables 1 and 2 are a word analysis and classification as has been explained. Such analyses were made of the records of each individual. The remainder of the tables are classifications of analyses demonstrated in Tables 1 and 2. Thus, the categories in Tables 3 and 4 are based on the word analysis in tables such as 1 and 2. For example, the words mouth, hair, etc. are placed under the column *Anatomical details* in Table 3. Such words as mother, father, man, etc. are placed under the column *People*. Bow, arrow, shoes, and the like are categorized as *Man-made material*. Such words as rocks, hills, rivers, wind, and others are classified as *Natural world*. Each column contains responses from all of the subjects.

TABLE 3

SUMMARY AND CLASSIFICATION OF STIMULI USED BY SUBJECTS FROM TEST

Card number	People	Anatomical details	Man-made material	Natural world
1	7	3	6	—
2	10	1	4	—
3	14	2	2	—
4	7	—	7	1
5	7	—	7	9
All Stimuli	45	6	26	10

TABLE 4

SUMMARY AND CLASSIFICATION OF STIMULI ADDED TO TEST STIMULI

Card Number	Human		Animal	Man made material	Natural world	Fighting or hunting	Other
	Family members	Other					
1	13	1	3	3	2	2	1
2	1	3	4	2	8	4	—
3	—	3	4	7	1	—	—
4	—	4	5	2	4	6	1
5	3	3	3	3	7	2	2
All Stimuli	17	14	19	17	22	14	4

MEANING OF THE RESULTS

In terms of our concepts described under our definition of perception it is necessary to examine the results in relation to the culture. Our first point was that of *selection*. We studied selection by examining the stimuli used and added to the cards. Thus, we can ask: what evidence is provided from the protocols as to the selection of stimuli from the environment and from their own experience? What objects and material are important to them in appraising and responding to a new situation? The records indicate

concern with human beings, simple things that individual human beings make, and the rocks, hills and rivers of their world. From the description of the setting and technology of their culture it was shown that the Cheyenne have few possessions and still live close to the land; and, in summer often live in tents and move about within their area. There are few man-made things and the people live close to the animal and physical world where trees, wind, and rain are part of their lives. The children show greatest selection of human stimuli and more will be said about this point later.

The next point we believed to be important for our study is that of the perception of *emotional experience* and the emotions associated with perceptive experience. The children in this study were able to give only a few expressions of emotion. And, the emotional expressions in evidence are with one exception those of a negative nature such as those of sadness and worry.

Our third point was in relation to *meaning*. The record of these children indicate that the meaning they assign to stimuli is mainly of a negative nature and that they see their world as hostile and depriving.

For the group, even human beings are seen as more often hostile than beneficent. There are no beneficent nonhuman forces. Of the nonhuman forces all are hostile with death, being lost, and the forces of nature being the most important.

As we stated at the beginning we cannot establish a direct cause and effect relationship between the forces of the culture and individual perceptual experience. However, we can say there is a coexistence between the conditions of deprivation and disintegra-

TABLE 5

EMOTION AND FEELING EXPRESSED

Card Number	Like (to)	Sad	Lonely	Worry	Tired	Angry (mad)	Sick
1	—	2	1	2	1	—	1
2	1	1	—	—	—	1	1
3	—	2	—	—	—	1	—
4	—	—	—	—	—	—	—
5	—	—	—	—	1	—	—
All responses	1	5	1	2	2	2	2

TABLE 6

EXTERNAL FORCES—HUMAN

Card Number	Beneficent		Hostile			
	Family	Non-family	Rejection	Fighting	White man	Enemy
1	2	—	—	—	—	—
2	3	3	2	—	1	1
3	1	—	2	—	—	1
4	—	—	—	2	1	—
5	—	—	1	1	1	—
All Human forces	6	3	5	3	3	2

TABLE 7

EXTERNAL FORCES—NONHUMAN

Card Number	Beneficent	Hostile					
		Death	Being lost	Parental separation	Deprivation	Dangerous Animals	Forces of nature
1	—	10	2	2	3	—	—
2	—	—	1	—	—	1	—
3	—	—	1	—	—	1	—
4	—	—	—	—	—	—	—
5	—	—	—	—	1	—	6
All nonhuman forces	—	10	4	2	4	2	6

TABLE 8

SUMMARY OF EMOTION EXPRESSED AND EXTERNAL FORCES

Emotion Expressed		External Forces	
Positive	1	Beneficent	9
Negative	14	Hostile	41
All categories	15		50

TABLE 9

STIMULI USED AND ADDED SHOWN IN PERCENTAGES

Category	Stimuli Used	Stimuli Added
Human	59	29
Animal	—	18
Man-made material	30	16
Nature world	11	20
Fighting or hunting	—	13
Other	—	4

tion in the security systems of a culture and the perceptions of the individual.

We see it as a part of our task to state our beliefs about the lack of certain security systems in the culture and suggest the effect we believe the lack has on adjustment. The sacred system of the Cheyenne is in a state of change—some clinging to the old tribal beliefs, some to the new, and some developing a mixture or a new cult. This confusion certainly could not be expected to provide much security for developing members. The family, another important security system, has also been shown to be precarious in its offerings. Divorce and separation out of economic necessity are frequent and thus we would expect to discover some effect in the security of the individuals. The family is important to these children, but there is concern over death and separation. The family does not give them the security needed. Our subjects do not express feelings of a positive nature toward other children or adults. It is indicated by the number of the responses devoted to the family members that they express the need for security from their parents, but do not have strong affectional bonds with them. There are no expressions of succorance, for example, by children for others and only a few by adults for others.

What kind of defense system in the face of insecurity and hostility have these children adopted? They have not developed an adequate defense system of either an external nor of a personal nature. They have not turned to objects or fantasy. Neither have they resorted to emotional expression as an outlet nor has interpersonal exchange been sought successfully. The main defense has been submission and denial of feeling. There is a tendency to denial of stimuli and a shutting out of perceptual experience. Needless to say such an approach offers little satisfaction to human beings. While disintegration of the culture and its security systems cannot be directly traced in its effect on the individual there is evidence from our study which would lead us to believe that deprivation and the lack of emotional satisfaction in the family is reflected in the perceptual experience of developing members of the society.

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SYMPOSIUM ON FREUD

The following papers were presented at a meeting held at the Veterans Administration Hospital in Palo Alto, California and an identical meeting at the Center for Advanced Study in the Behavioral Sciences (Stanford, California), in commemoration of the one hundredth anniversary of the birth of Sigmund Freud.

History, Psychiatry, and the Behavioral Sciences

by Henry von Witzleben

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I CONSIDER it a great honor to be invited to speak before this distinguished audience on the celebration of Freud's 100th birthday. The fact that the members of this Center which is dedicated to the study of human behavior wish to pay tribute to the "erratic genius"—as Benjamin Lee Whorf has called Freud—proves again that psychoanalysis has branched out into almost all fields of science.

I am also pleased to share the honor with my other colleagues. It seems to me a particularly fortunate combination: two psychiatrists trained in psychoanalysis, a psychologist who has done much to narrow the gap between psychology and psychoanalysis, and a medical historian experienced in the history of psychiatry. As far as I know Dr. Veith[1] is the first University-appointed historian of psychiatry who takes Freud and psychoanalysis seriously and understands that dynamic psychiatry has much to offer to the historian. The reverse is also true: the psychiatrist can learn much from the historian, and this explains the origin of this paper. Some time ago Dr. Veith and I became interested in the history of psychiatric ideas in the Western World, i.e., an investigation in the way these ideas develop and influence the life of human beings, an investigation in the "adventure of ideas" to use Alfred North Whitehead's words.

Very soon we realized that first of all we had to clarify certain concepts. What does history mean to us? What are the relations between history, psychiatry, and the behavioral sciences? We presented the results of our considerations in a somewhat different way in 1955 at a meeting at the Veterans Administration at Palo Alto where they were discussed by Franz Alexander, Clyde Kluckhohn, and Fred Polak, all three members of the Center at that time.

As a psychiatrist I am of course curious to find out the motive that made us decide to take up this kind of work at the present time. Perhaps it is an escape from our age of unreason that has reached an unbelievable state of destruction and confusion, of stupidity, of suspicion of education and erudition. Social disorder and emotional disturbances are the consequences, as is the charlatanism in literature and politics, in art and philosophy, as is the threat to freedom and liberty.

The history of ideas or the "adventure of ideas" is a fascinating subject What it is concerned with is not only ideas as such, but the way in which ideas develop and influence the life of human beings.

If psychiatry is that branch of medicine which deals with the psychopathology of human behavior, which it tries to describe (descriptive psychiatry), to explain (analytic or dynamic psychiatry), and to correct (analytic psychotherapy), then a history of psychiatric ideas, that is a cultural history of psychiatry, cannot be limited to the description of systems and classifications, of the clinical picture of "insanity." Such a history has to investigate everything that contributes to the clarification—perhaps eventually

[1] See contribution by Dr. Veith, p. 67.

to the solution, of problems concerned with so-called normal as well as with emotionally disturbed persons.

Today the word "psychiatry" has broader implications than in the past, in that the discipline has joined hands with other disciplines such as psychology, anthropology, sociology, and more recently, history. We make every effort to learn from such disciplines and to contribute with them to the common pool of research on problems of human behavior.

The birthplace of psychiatric ideas is not the hospital or laboratory but the civilization and culture of societies, their mythology, religion, philosophy, and art. One cannot present a history of purely psychiatric ideas. Instead, one presents psychiatric ideas as they appear within a culture, making constant use of history and other humanistic disciplines.

It is my purpose to present some fundamental concepts which are necessary for the understanding of the relationship of psychiatry to history and the behavioral sciences, concepts which will help us later to understand the development and interpretation of psychiatric ideas. The wealth of the subject matter allows only for a very streamlined exposé, leaving many important questions unmentioned.

In our effort to contribute to the history of psychiatric ideas we are much more interested in what we call today "cultural history"; we are less interested in what usually is understood by history: the accumulation of more or less dry facts. We also wish to avoid as much as possible a discussion of purely philosophical problems, although we realize that this is not entirely possible. The term "philosophy of history," as introduced by Voltaire, originally meant nothing but critical or scientific history. It almost immediately lost this meaning under the devastating influence of German philosophy, or better, philosophical speculation. It is significant that the voice of one of the most erudite and cultured men Germany has ever produced remained unheard; Wilhelm von Humboldt's beautiful essay on the "Task of the Historiographer" (published in 1821) was unable to stem the flood of speculation which had already led the philosophy of history into Hegel's universal system and into the romantic philosophy of Schelling and other Romanticists. Later the pendulum swung to the other extreme: philosophical positivism tried to prove that history is in no way different from the so-called exact natural sciences, and imposed the uniform laws of natural science on history. The most influential representative of this school was Hyppolite Taine.

That we as psychiatrists and psychoanalysts are particularly interested in the psychological aspect of history goes without saying; but historians also realize more and more that the days are gone when the goal of the historian was—according to Ranke's famous statement—to find out "how things really were." Historians rather ask now "how things came about," that is, they are interested in the dynamic interaction of events.

As psychiatrists we are not qualified to make a judgment on the many specific problems of the historians. We are only partly interested in whether or not history is a science. For our purposes it is sufficient to state that history is certainly not a science in the same way that the natural sciences are, that is, the universal laws of natural science are not applicable to history. In a natural process the dead past is replaced with something new, present. In historical process, the past is not dead. It and its processes survive in the present. But the aim of any science is to find things out, and in that sense history is a science. It is a science concerned with human actions in the past. The evidence of this science must be, and is, different from the evidence of other sciences, but history has one thing in common with all the sciences we know: the interpretation of evidence. The interpretation of historical evidence—and here we agree with Collingwood, who in general does not feel too friendly toward psychology—serves human self-knowledge. "The value of history is that it teaches us what man has done and thus what man is" (Collingwood).

If then, history is a part of the sciences concerned with human nature, it must be closely related to other sciences of that kind, for instance, psychology, psychiatry, and

the behavioral sciences. It is hard to understand that there are still historians who refuse to accept this fact; on the other hand there are also, unfortunately, psychiatrists who feel that history has validity only insofar as their own concepts are applied. It does not occur to these psychiatrists that they could learn something from history and even historians.

It has already been briefly mentioned, and I wish to repeat it, that history is a part of the science of human nature, but that history cannot reach its goal by using laws only applicable to natural sciences. The attempt of the eighteenth-century English and Scottish School to understand the human mind has failed; Locke and Hume have ended in a dead-end road. They failed not only because they could not apply psychological viewpoints (there was no psychology in our sense of the word at that time), but even more because the concept of history as the reconstruction, analysis, and interpretation of the past did not develop before the middle of the nineteenth century.

We also cannot accept the view of the other extreme school of thought, namely, that there is nothing but history; that history is, so to speak, the basic science. It was not only Benedetto Croce's stupendous mind which created the omnipotence of historical science and the autonomy of history; we find the same ideas in others and read, for instance, in a brillant essay of Ortéga y Gasset, that "Man has no nature, he has history."

For us, interested in human behavior, the most important difference between natural processes and historical processes, is that we can describe natural processes as a sequence of events; history, on the other hand, does not consist of sequences of events but of a sequence of human actions. Collingwood is quite correct in pointing out that these historical human actions have an outer and an inner side. He is certainly not correct in saying that the inner side of the historical fact is a process of thought and that, therefore, all history is the history of thought. We cannot accept this way of philosophical, deductive thinking. For us the inner side of a historical fact—be it political, economic, or an idea—is something psychological, emotional, no matter whether it is conscious or unconscious. Or, in nonpsychological terms: "history is the spiritual form under which a culture gives itself account of its past" (Johan Huizinga).

At this point we find ourselves in the position where we cannot agree with the two most extreme definitions of history: one, that history is nothing but applied natural science; or two, that there is no other science but history. Now we must look for another solution of the problem. This is not an academic question but a very practical one, because, unless we can define what history is and how we as psychiatrists can make use of it, we will be unable to investigate our problem of the development and interpretation of psychiatric ideas.

It seems that there is no other way but to accept a certain point of view which rather disparagingly, and in my opinion wrongly, has been called relativism.

We have learned from modern microphysics that the answer to a question depends on how and when a question is asked; in other words, that he who conducts the experiment has a definite influence on the outcome of the experiment and that there may be more than one possible "true" answer. If we want to apply this concept to history, it means that we have to discuss what is called historicism, which may be a way out of the dilemma. We very generally can define historicism as "clothing the dry facts in their cultural milieu."

I do not believe that there are universal values in science; I believe that man's knowledge and general philosophy are rooted in the mental climate of his generation; that man is shaped by two factors: his individual emotional make-up and the society and culture in which he lives. This is the main reason why history has to be rewritten ever so often.

I know how much my own way of thinking was shaped by the mental climate of my generation, that is, the revolutionary generation of Central Europe after the first World War. What is characteristic for that generation is the tremendous drive away from specialization, from detailed and endlessly drawn out analyses. It is the drive toward life as a whole, toward synthesis; synthesis

in philosophy, the history of ideas, in art and in literature. We find the same urgent needs in psychiatry, beginning with Jasper's introduction of phenomenological viewpoints in 1911. Later these drives manifest themselves in the introduction of psychological aspects of, for instance, schizophrenia. We find the influence of psychoanalysis and dynamic thinking constantly increasing. It was psychoanalysis which pointed out the importance of the concepts of structure and function. It must be kept in mind that the term "psychoanalysis" as created by Freud, has two parts, analysis and integration. Analysis is only the vehicle we use to reach our goal, which is synthesis. What we do in analytic therapy is to make the patient integrate the irrational into the rational, through the medium of what Alexander calls the "corrective emotional experience."

The nucleus of the theory of historicism is that history and historical knowledge can be understood only with reference to a defined system of coordinates. This frame of reference is the setting of an historical process at a certain time and in a certain society. It is therefore necessary to ask not only what is said in an historical event or process, but also why it is said or why it happened. Historicism fully accepts the process of change, including the change of values, and historical judgment is certainly value-charged.

Knowledge is only possible with reference to its place in an historical process. Troeltsch introduced this statement in 1924 in his epoch-making book on *Historicism and Its Problems*. This kind of knowledge makes it possible to describe and evaluate past epochs in terms of their own standards and values, a process which you might call "understanding" or "intuition."

Our next question must be: what is the relationship of psychology to history? This question is not new and there have been different answers to it. One of the answers was propounded by Karl Lamprecht at the end of the nineteenth century when he made the first systematic attempt to apply psychology to history. He became interested in psychology through his friendship with Wundt who founded the world's first laboratory for experimental psychology in 1879 (the second was opened at Harvard one year later under William James). Of course Lamprecht knew only the academic psychology of his time. He knew nothing about Freud, but in one of his papers he makes a surprising statement about unconscious processes and their value for history. This was before Freud had published his first analytic papers.

For Lamprecht, history is a "socio-psychological science," a collective psychology. We cannot discuss his teachings here, but I think it is only fair to mention at least the name of the first historian who made a systematic attempt to apply psychology to history.

We should remember that science at Lamprecht's time was still much under the very strong influence generated by the theory of evolution. Evolution was supposed to explain everything, and whole systems of philosophy, psychology, and sociology were built on the idea of evolution, for example, Spencer's philosophy and psychology. Not that evolution does not exist or that man is not the final result of a long evolutionary process. We do not contest the fact; we only wish to point out the fallacy. The fallacy of the philosophy of evolution is that it automatically, and without any criticism, includes the idea of value-charged progress. Without making this point very clear we might fall into the same trap when we try to apply dynamic psychology to history. We find all the evolutionary philosopher-historians possessed of the idea of progress and a superficial philosophy of optimism, the philosophy of "bigger and better." It seems as though, dynamically speaking, the belief in progress is the emotional reward for the hardship of life.

This mistake we must avoid in our psychological approach to history, and can perhaps do so by distinguishing between culture and civilization. Culture is a psychic-emotional phenomenon which has its own rules of development. Only civilization (in this country sometimes called "material culture") can be described in terms of progressive evolution; culture has a constant change of meaning; a change of meaning does not exist for civilization. It is constant in the same way that Euclidian geometry means

the same for us as it did for the Greeks, and as a hammer has the same meaning for a Zulu tribesman and an American woodsman. This is not true for cultural phenomena. The cult of Aesculapius, for example, meant something entirely different to the Greeks from what it means to us.

Psychologists and psychiatrists, and this includes psychoanalysts, are too easily inclined to apply their theories generally and schematically to other fields without considering that other fields have a different structure and function. Our psychological knowledge of the individual cannot simply be applied to the psychology of groups, societies and cultures. This is the basic mistake so many psychoanalysts have made, although others such as Alexander, have warned that it is not possible to take Freud's cultural and anthropological theories (totem and taboo, the primal horde, etc.) for granted, and in this way "explain" history and culture.

The psychology of the individual and the psychology of societies are two different things which are not interchangeable. However, they have one thing in common, the fact that both normal and emotionally disturbed individuals and groups are unaware of their motives and their dynamic forces. The actions of both individuals and groups are not determined by one single motive, but by many different complex ones.

The reluctance of historians to accept psychology was understandable at a time when there were so many brands of psychology that the historian did not know where to begin to find a useful one. Today the situation is easier, since there is only one kind of psychology that is helpful to the historian, dynamic psychology. But dynamic individual and social psychology can be helpful only within the context of the behavioral sciences.

Ever since anthropology broke away from what we can call "bone anthropology" and developed into cultural and philosophical anthropology, it has become so closely related to sociology that it is not always possible to differentiate between the two sciences. Not only have anthropology and sociology been of great interest to the psychiatrist and psychoanalyst (I mention only the names of Freud, Abraham, Rank, Roheim) but also psychiatry has had a very decisive influence on both disciplines. This has led to a mutual infiltration of ideas, and even, lately, to a very close collaboration from which a history of psychiatric ideas will greatly benefit.

I cannot go into the history of this movement which started in England with Malinowski and Seligman. The two outstanding anthropologists of the early years of American anthropology, Boas and Kroeber, were convinced of the importance of psychological and psychoanalytic theories for anthropology. For us psychiatrists the most astonishing figure of all of them is Edward Sapir, the real founder of American psychological anthropology. Sapir had not only a thorough critical knowledge of psychiatry and psychoanalysis, he was also unusually gifted in his ability to convey dynamic thinking to his colleagues, using the English language in a way one wishes he could find more often in scientific literature.

The purpose of anthropology and sociology is to help us to understand human nature. Psychiatry deals with human nature within its culture. The credit for the distinction between society and culture goes to the anthropologists. But we immediately hit difficulties as soon as we try to define what culture is. That it is something psychic-emotional is not sufficient. For Kluckhohn, "Culture is not behavior, and behavior never culture, but is a logical construct." For the psychiatrist I think it does not make too much difference if we accept this definition or look at cultural processes from the point of view of realistic or idealistic schools. I feel that we can be eclectic and consider that each of these different statements is helpful for our understanding of the development of psychiatric ideas. Personally, I am inclined to be on the more idealistic side, since I believe in the distinction between culture and civilization, whereas the realists include material and non-material products in their definition of culture. I also cannot see the importance of the more or less hair-splitting discussion of culture as something organic or super-organic (in the sense of Spencer). Of course, culture is something organic, for the simple reason that it satisfies psychobiological needs, and I do not deny

at all that, as Sigerist has repeatedly pointed out, biological constitution and health or disease are very important factors in any cultural process. What I do deny is that biological factors are something primary which would lead logically to the "myth of the twentieth century," racism, and "blood and soil."

Another question which should be clarified is: how much of our analytical experience can we apply to cultural processes? Again it seems to me that analysts are a little too careless in such applications, taking for granted Freud's statement: "We have recognized the evolution of culture as a special process comparable to the normal growth of an individual to maturity" (*Civilization and Its Discontents*). This implies not only again the dangerous theory of progress, but it states that there is more than mere similarity between such cultural phenomena as myth and rituals and the emotional processes we find in children, neurotics, and psychotics. For example, the "primitive" state of development of a child or a neurotic, or the psychotic regression to a more primitive level, is different from what we call "primitive" in the cultural sense. The statement also overlooks an important fact mentioned at an earlier occasion: in analysis we deal with individuals. If we talk about primitive people we mean a group, and it is not correct to transfer individual experience to groups. The very few attempts at individual therapy with so-called primitive people made quite recently cannot change our attitude in this respect (Devereux).

More familiar to all of us is the opposite idea: that the individual development recapitulates the cultural development. This idea we use often as a working hypothesis, but we should not forget that there is no proof for it, and that there will be no proof as long as the touchy problem of a "racial memory" and the inheritance of acquired characteristics has not been solved.

We do not deny—we rather accept as a fact the psychic unity of mankind and the universality of the basic structure and function of the mind, that is, the universality of the dynamics as proposed by Freud. In their interesting work on dreams, Seligman and Lincoln have shown that there are universals in the dreams of individuals belonging to different and widely separated cultures and societies. We agree with Seligman's statement: "If it is found that savage dreams conform to constitution, and function to the explanations, offered by psychologists working on dreams of folk of western race and culture, we may well be prepared to accept as actually demonstrated the claim that the savage mind and the mind of western civilized man are essentially alike." If this is correct, it is only logical that dynamics such as repression, introjection, projection, etc. should be universal, even though their appearance is different because they are colored by two factors: the biological cerebration and the socio-cultural setting.

The psychic unity of mankind and the universality of dynamics are of decisive importance for the way in which we can conduct our investigation of the development of ideas. To avoid misunderstandings I should make it clear that Roheim's original autogenic theory of culture with its rigid orthodox acceptance and applications of Freud's anthropological and sociological theories is unacceptable to me. It is Roheim's one-sided reduction of culture to psychology to which I object, his dogmatic statement that "psychoanalytic thinking is reductionist." This means that all cultural phenomena can be explained by reducing them to symbolizing unconscious psychological processes. A social and cultural system is not alone the result of and reaction to actions of individuals; it is in itself a structural-functional system of action.

What I have tried to show in this paper is how psychiatry and psychoanalysis, and this includes the history of medicine and psychiatry, are related to certain problems of general history and the behavioral sciences. There is no doubt that whatever I can say in a limited time is incomplete, but I hope that what I have said and what the discussants will add in their criticism and suggestions at least will clear the way to a certain extent for what Dr. Veith and I will try to accomplish: to describe, understand, and interpret the history of psychiatric ideas, as a small contribution to the problem of human behavior.

As a young man I always refused to accept Freud's pessimism which boils down to the statement: man is no good. After having experienced two world wars, dictatorships of all kinds with their goal of destroying freedom and liberty, I am ready to admit that Sigmund Freud was a very wise man. The youthful optimism is gone or at least badly battered, and I am afraid that digging into the history of ideas will not restore optimism but confirm what Wordsworth said:

> For I have learned
> To look on Nature not as in the hour
> Of thoughtless youth; but hearing often-times
> The still, sad music of humanity.

The Primary Processes and Psychoses

by Henry W. Brosin

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IT is fitting that we meet in commemoration of Freud's birthday in view of his importance to our field and the world of thought at large. It is also noteworthy that we can honor him most by discussing some of his ideas in terms of lively controversy on several new clinical research frontiers rather than by limiting ourselves to a eulogy of past accomplishments. For it is an astonishing fact that sixty-three years after he started writing (1893) on psychological subjects and seventeen years after his death (1939), his ideas remain a popular center for renewed stimulation to new experiments and hypotheses about human behavior. It has been said that with the current acceleration of investigative work that "the half-life of a scientific idea is rapidly growing shorter," but this decline is not yet apparent in the most stable of Freud's contributions.

Although Freud was skeptical about the power of psychological treatment for psychotic patients, several daring investigators demonstrated that with proper techniques and insights these patients could also be understood and helped back to health (2). It must be said that this possibility was appreciated by psychiatrists in the early nineteenth century, but did not have strong support during the latter nineteenth or early twentieth century (25).

The return to a more active psychotherapy for psychoses was strongly urged by Eugen Bleuler (1) and has reached a high point in the demonstrations by Harry Stack Sullivan and Frieda Fromm-Reichmann. Slowly but definitely the psychological hypotheses for the treatment of psychoses are gaining more credence and interest all over the world and therefore deserve a brief review in this hospital setting.

The Primary Process

Perhaps our inquiry can best begin with a condensed review of Freud's conception of the unconscious where our mental life has its origins and where it is a most potent governor of the destinies of all of us, including the psychotic. Freud says in a summary in the essay "Beyond the Pleasure Principle":

> The fact that the sensitive cortical layer has no protective barrier against excitations emanating from within will have one inevitable consequence: viz., that these transmissions of stimuli acquire increased economic significance and frequently give rise to economic disturbances comparable to the traumatic neuroses. The most prolific sources of such inner excitations are the so-called instincts of the organism, the representatives of all forces arising within the body and transmitted to the psychic apparatus—the most important and most obscure element in psychological research. Perhaps we shall not find it too rash an assumption that the excitations proceeding from the instincts do not conform to the type of the "bound" but of the free-moving nerve processes that are striving for discharge. The most trustworthy knowledge we have of these processes comes from the study of dreams. There we found that the processes in the unconscious systems are fundamentally different from those in the (pre)conscious; that in the unconscious "charges" may easily be completely transferred, displaced or condensed, while if this happened with preconscious material only defective results would be obtained. This is the reason for the well-

known peculiarities of the manifest dream, after the preconscious residues of the day before have undergone elaboration according to the laws of the unconscious. I termed this kind of process in the unconscious the psychic "primary process" in contradistinction to the secondary process valid in our normal waking life (8).

While much of the meaning of this passage is familiar to you, it is probably true that the additional material provided by the Fliess' letters, the Project for a Scientific Psychology, the biography by Ernest Jones, and the careful examinations by Hartmann, Kris, and Rapaport have helped us in our grasp of the many assumptions implicit in this difficult area (20).

If we collate the numerous statements by Freud about the activities of the primary process from its origins to its transformation into the secondary system, we find that he at first envisioned a neurophysiological model of the organism in which inner drives seek discharge outward toward an object. If the tension mounts sufficiently high and the object sought after is present, then a discharge of tension occurs and the organism becomes quiescent (9). With frustration of this pre-primary process discharge, the organism develops ways in which to gain satisfaction until external discharge is again possible. The blocked discharge is spread into the soma (affect discharge) and there is also the appearance of an hallucinatory image recreating the desired object and thereby providing at least temporary satisfaction. This image, called "ideation" by Freud, is thought of as the forerunner to genuine thinking and is also an early example of wish-fulfillment. These processes are known to us empirically in toxic and organic states as well as states of disordered metabolism such as dehydration, hypoxia, starvation, and from the studies of dreams and hypnagogic experiences. They furnish a substantial bridge for comparison with the hallucinations of psychotics and aid enormously in understanding them. We now know that the mechanisms of condensation, displacement, and secondary elaboration described in *The Interpretation of Dreams* has much wider usefulness and Freud urged that these other states of consciousness be studied in order to create a worth-while psychology.

But as soon as we endeavor to penetrate more deeply into the mental process involved in dreaming, every path will end in darkness. There is no possibility of *explaining* dreams as a psychical process, since to explain a thing means to trace it back to something already known; and there is at the present time no established psychological knowledge under which we could subsume what the psychological examination of dreams enables us to infer as a basis for their explanation. On the contrary, we shall be obliged to set up a number of fresh hypotheses which touch tentatively upon the structure of the apparatus of the mind and upon the play of forces operating in it. We must be careful, however, not to pursue these hypotheses too far beyond their first logical links, or their value will be lost in uncertainties. Even if we make no false inferences and take all the logical possibilities into account, the probable incompleteness of our premises threatens to bring our calculations to a complete miscarriage. No conclusions upon the construction and the working methods of the mental instrument can be arrived at or at least fully proved from even the most painstaking investigation of dreams or of any other mental function taken in *isolation*. To achieve this result, it will be necessary to correlate all the established implications derived from a comparative study of a whole series of such functions. Thus the psychological hypotheses to which we are led by an analysis of the processes of dreaming must be left, as it were, in suspense, until they can be related to the findings of other enquiries which seek to approach the kernel of the same problem from another angle (12).

We will examine some of these experimental psychoses after we complete our survey of the primary processes and the transition to logical thinking.

With the mobile energy charges attached to free-floating ideation, Freud also postulated that all ideas attached to the same drive are equivalent and hence interchangeable. Past, present, and future are simultaneously present for representation. Space coordinates are not fixed; space can be used to represent a measure in time; for example, something far away represents something which happened long ago. There are no direct ways to represent negation. Multiplicity of figures may represent repetitive acts (10). These properties describe the transformations of a drive in its effort to find fulfillment when direct discharge via action is delayed. As the delays become more

frequent and more internal, Freud suggests that small amounts of energy are utilized for an experimental search for satisfaction. As this method is built up, there are established new agencies or instruments for both inhibition of the basic drives and the means by which they find satisfaction. These new devices are variously known as ego functions, secondary or logical process, or rational thinking. With their development we are able to see such new functions as a new memory organization independent of those centered around a single drive, a more selective perception, better coordinated motor activities, in short, an integrated ego.

This description, although brief, offers a basis for asking concrete questions which may be answered by experiments sooner or later, and which will affect our current views about diagnosis, prognosis, and therapy. For example: Are the potentialities for some ego functions inherited? Freud had no doubt about it.

> We have no reason to dispute the existence and importance of primary congenital variations in the ego. . . . But we must not overlook the fact that id and ego are originally one, and it does not imply a mystical over-valuation of heredity if we think it credible that, even before the ego exists, its subsequent lines of development, tendencies and reactions are already determined. The psychological peculiarities of families, races and nations, even in their attitude toward analysis, admit of no other explanation. . . .
>
> When we recognize that the peculiarities of the ego which we detect in the form of resistances may be not only acquired in defensive conflicts but determined by heredity, the topographical differentiation between ego and id loses much of its value for our investigations (11).

It seems to me that while modern geneticists are far from dealing with such problems in humans they are evolving methods in population and comparative genetics and twin studies to help the eventual solution of these questions.

The ethologists such as Liddell, Lorenz, Tinbergen, Hess, Thorpe, Scott, and Fuller are demonstrating early action patterns in animals which will be meaningful in human experimentation on "instinctual" behavior if appropriate precautions are taken (Gardner Murphy, T. C. Schneirla). Freud's forthright assumptions are meeting experimental verification at the animal level and the prospect for experimental genetic exploration into such human concepts as intelligence, health, work ability is improving daily (13). It is possible that improved methods in social psychology and population genetics will bring us sound information about such concepts as potentiality for guilt, aggression, castration, or ego integrity.

Freud hinted in the last sentence quoted that the primary processes are not discrete entities but rather form the poles of a continuous spectrum (14). Rapaport has unpublished clinical data to corroborate the view that the dichotomy between primary and secondary processes will be abolished with a concept of gradual transitions (19).

Other workers in clinical conditions which exhibit various states of consciousness are well known to you since most of this work was published by Freud: Schroetter (1911) with hypnosis and dream symbolism; Betlheim and Hartmann (1924) with Korsakoff's syndrome; Silberer with hypnagogic experiences; Schilder with paretics.

Rapaport also mentions Meynert, Schilder, and Hartmann's studies in amentia, and Janet's work with fugues and amnesias.

To this list we can now add Charles Fisher's studies on dream symbolism as revealed in a repetition of Poetzl's experiments on dream production following tachistoscopic exposure to pictures (6). The second series of Fisher (7) involve a study of image formation and the relationship of the latter to the construction of manifest dream images by a modification of the methods used by Allers and Teler. He was able to confirm

> Poetzl's essential finding that those parts of the tachistoscopically exposed picture which are not consciously perceived appear extensively in the manifest content of subsequent dreams. . . . Based upon Poetzl's original findings, [Fisher] was able to confirm the fact that in the process of dream formation, these preconscious percepts or their memory images undergo a number of transformations and distortions such as condensation, displacement, fragmentation, spatial rotation, symbolic transformation, etc. These various distortions and transformations are utilized by the dream work and a reciprocal relationship exists between them and the unconscious processes which enter into dream formation. It was sug-

gested that the preconscious visual percepts or their memory traces, associated with the day residue experience, are worked over by the primary process and the unconscious wish at the moment of tachistoscopic exposure or in close temporal relationship to it. These percepts play a significant and elaborate role in the transformation of the dream thoughts into plastic visual images, that is, in relation to what Freud calls, "regard for plastic representability."

I will not try to mention many important studies by experimental psychologists such as Sears, Miller and Dollard, Mowrer, and many others who are adding to our information since E. R. Hilgard will probably cover this ground in his address.

I will take this opportunity to point out that a vast new range of experimental situations with human patients are available to us with the advent of the new agents. Metrazol, insulin, heat and freezing treatments, psychosurgery in its many forms, and now the so-called tranquilizing drugs (Serpasil-reserpine and chlorpromazine-Thorazine), are providing ample human subjects for the study of the effects of stress upon the organism. Unfortunately, few of these patients are being studied with the care which they require from a scientific point of view. However, there is one consistent conclusion which is now appearing more regularly in reviews of the physio-dynamic therapies and that is that "they tend to be inadequate unless the gains they provide are consolidated by psychotherapy" (23). We will probably become more skilled and better motivated to the difficult study of the role of the highly important unconscious functions in psychotherapy as we become more familiar with them.

New Experimental Approaches: Studies in Isolation, Decreased Sensory Input or Sensory Deprivation.

In addition to the experimental and clinical work cited to demonstrate that an interest in the concepts of the "primary process" are very much alive currently, I would now like to mention very briefly, since full exposition is not yet available even if time were, the experiments reported by Hebb and his associates (16), and those of John Lilly (17) together with some associated material (4, 22, 24).

While Hebb was doing studies on boredom at routine tasks during 1952–54, he paid male college students to remain quietly on a comfortable bed in a cubicle 24 hours a day. Only 14 subjects are reported out of a total of 46. Most subjects quit from within a few hours to 72 hours in spite of the fee of $20 per day. Goggles which only admitted light were worn, along with cardboard cuffs extending beyond the fingertips, thus permitting free movement but limiting tactual perception. The subjects were able to communicate freely with the experimenter through a speaker system and there were no problems regarding meals, toiletry, etc. Standard problem-solving tests were given before, during, and after their entry into the cubicle, and also to control subjects at the same intervals.

The results show impairment in problem-solving while in the cubicle. . . . Subjectively, also there are reports of inability to concentrate. On leaving the cubicle, there is apt to be some defect of sensori-motor control, and a mild state of confusion. . . . The most striking result, however, is the occurrence of hallucinatory activity, chiefly visual but also auditory and somesthetic in a few subjects. The phenomena are quite like what is described in mescal intoxication, as well as what Dr. Grey Walter has produced by exposure to flicker. Also there are rare cases of hallucination in aged persons without psychotic tendency, which as with our subjects does not depend on special stimulation (our method is more like a lack of stimulation) or chemical agents.

I will not attempt a detailed account of the progressive stages which seem to make the development of the more advanced hallucinatory states. Students of the "primary process" in dreams and other human behavior will have no difficulty in recognizing these familiar activities in this new setting. Hebb has an interesting comment.

It appears to show that normal functioning by the human brain is much more dependent on varied sensory input than we have suspected in the past. Even though sensory connections are intact, and the sense organ being stimulated at a level of intensity within the normal range, if this stimulation is monotonous instead of being normally varied there is soon evidence of intellectual deterioration.

John Lilly has similarly created a situation for sensory deprivation by putting a subject

into a tank of water at 34.5°C. wearing only a blacked-out headmask for breathing. The goal here is to reduce the absolute intensity of all physical stimuli to the lowest possible level. Only the experiences on two subjects are reported and for 3 hours' exposure (17). Lilly reports seven stages of decreasing control of the secondary process and increasing evidence of the "primary process" activity culminating in vivid, three-dimensional visual hallucinations. After an extensive review of many reports of explorers and solitary sailors, he concludes that

> If one is alone, long enough and at levels of physical and human stimulation low enough, the mind turns inward and projects outward its own contents and processes; the brain not only stays active despite the lowered levels of inputs and outputs, but accumulates surplus energy to extreme degrees.

J. A. Vernon of Princeton has constructed a soundproof, darkened room which is thoroughly insulated for the purpose of decreasing sensory input. He has kept his subjects in the room for 24 hours and reports some deteriorated behavior but no delusions or hallucinations (24).

There are isolated reports of experiments, which are not yet verified, that some selected psychotic patients recover by studied treatment with isolation. This also makes sense if one recalls the special integrating functions reported in religious retreats and other techniques of silence and isolation.

Stanley Schachter of the University of Minnesota has studied reports of hermits and others living in isolation and concludes that the setting makes a big difference in the results (22).

The paradoxical effects of isolation are seen in profuse confusion in reports by prison physicians. Some prisoners, such as informers, seek occasional isolation as a refuge from assault; others seek isolation as an opportunity to defend themselves against a threatened psychosis; some apparently enjoy isolation since they have mastered techniques for elaborating a pleasant phantasy life such as described by Jack London in his book, *The Star Rover*. Since prisoners are put into isolation for punitive reasons, it is only to be expected that their reactions will vary with their personalities, life histories, and the current setting. In any event they will differ in some ways from college students who are subjects in a laboratory.

Time does not permit review of the hallucinatory states induced by mescaline or lysergic acid (D-lysergic acid diethylamide or LSD 25). The latter drug in very small doses tends to produce schizophrenic-like symptoms in human beings. It is notable that "psychotics in general display a far greater resistance to the somatic and psychic effects of LSD 25 than normal subjects" (23). Sandison reports "There is a sense of depersonalization, or of division of the personality, the conscious watching the unconscious. In contrast to this decay of reality the patient's vision of the unconscious is solid, vivid and dynamic" (21, 23). He also states that "the experiences of psychoneurotics under LSD 25 reveal a striking similarity to the dream and fantasy material of patients undergoing deep analysis." Other workers are not so enthusiastic.

Cattell gave mescaline to three categories of schizophrenics which affected the dynamic material sufficiently to be a useful technic for investigating personality structure without having any apparent value as a therapeutic agent (3, 23).

This brief survey documents the view that intensified study of the primary and secondary processes will be more prominent in the coming decades with greater insights into the psychotherapeutic processes and their cousins, the phenomena known as "brain-washing" or "brain-changing" made familiar by prisoners from the Iron Curtain countries. It is also a matter of optimism that new hypotheses regarding ego development by psychiatrists and social scientists such as H. Hartmann (15), Erik Erikson (5) and H. Marcuse (18) may stimulate us to new therapeutic efforts.

But with or without a current comprehensive theory it is apparent that many therapists and investigators are gathering empirical experience which should make possible increasingly better understanding of human behavior. Not least among our improved insights is the increasing awareness of the importance of the total setting

of the patient-subject and the observer in a psychosocial matrix for which we again owe thanks to Freud.

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Freud's Place in the History of Medicine

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IT is indeed a pleasure to be included among the celebrants of the hundredth anniversary of Freud's birth. I am truly proud of this honor which has been extended to me, a non-analyst—and quite awed by the great occasion. When I refer in this connection to Thomas Mann's oration in Vienna on the occasion of Freud's eightieth birthday, I do so only because I wish to borrow one of his statements which so thoroughly describes my own feelings. Mann said: "to talk about [Freud] I hardly dare—what is there new about him that *I* could hope to tell the world."[1] Mann solved this dilemma by talking mainly about himself and his own works in relation to analytical concepts. I have decided to solve it by speaking about the medico-historical setting of Freud's earlier days.

Thus my expression of modesty like that of Mann must be taken with a grain of salt, for, after all, Mann had accepted the assign-

[1] Mann, Thomas. *Freud und die Zukunft*. Wien, 1936, p. 15.

ment of the Freud oration, and other literary men who succeeded him in similar tasks are entirely free from any real or pretended modesty and appear quite certain of their right to speak about Freud and his work. With the exception of the physical sciences there is hardly a branch of scholarship and science that cannot and does not make a claim on Freud, and numberless exponents of disciplines far removed from psychiatry and medicine will doubtless be cognizant of this anniversary.

This is in significant contrast to a similar celebration that took place two years ago when homage was done in Frankfurt, Germany to the founder of chemotherapy, Paul Ehrlich. There the celebrants consisted mainly of physicians, chemists, and biochemists, and it appears that few persons outside the realm of the biological sciences were still aware of the personality that had created the "magic bullet."

To be sure, much of the practice of modern medicine rests on the foundations laid by Erhlich, and it is questionable whether the "wonder drugs" could work their miracles if the "magic bullet" had missed its aim. Like the wonder drugs, psychoanalysis has become part of the intellectual property of modern man; but unlike the wonder drugs whose originator has become all but lost in the maze of history, analysis remains intrinsically linked with the name of its founder. A Greek epigram said about Plato, "Wherever I go in my mind, I meet Plato coming back." I believe I am not exaggerating too much if I adapt this proverb to our day by saying, "Wherever I go in my mind, I meet Freud coming back."

In view of the tremendously widespread awareness of Freud among the literary figures of our day, the social scientists, the educated public, and even the medical profession, it is astounding that medical historians have so far been rather neglectful in their study of psychoanalysis and its founder. This failing can certainly not be attributed to the relative recency of the analytic movement, for much more recent events such as the work of Ehrlich, Banting, Fleming, and even Salk have been deemed worthy of historical perspective.

The relative neglect of recent and even older psychiatric developments in medico-historical thinking appears neither conscious nor deliberate. It is simply traditional; and I, having grown up in this tradition, might have abided by it with complete unawareness of its existence, had not Henry W. Brosin pointed it out to me several years ago as a peculiar and disturbing phenomenon. My reaction to Dr. Brosin's observations was startling—at least to me—he may be used to his own great impact—for my interest turned almost immediately to the study of the history of psychiatry, and at the same time, I began to examine the factors that had removed it from the attention of my colleagues.

These factors appear to be the following: medical history has generally taken its cue from medicine proper; it is—to use a somewhat strange metaphor—a sort of a pilot fish to academic medicine whose field of vision is obscured by the great fish of medicine swimming above its head. Since neither analysis nor its founder has ever been an integral part of academic medicine, they rarely attracted the attention of the pilot fish, namely the medical historian. Medical history is wont to deal with physicians, rather than with "physicians in spite of themselves," and although Freud had received the training of a physician it was evident to his colleagues and prospective historians that the development of his specialty was conditioned less by the then accepted medical theories, but rather by the social and literary atmosphere of his day, which is, we should not forget, the Viennese bourgeoisie of the Victorian era. Moreover, in spite of his sincere veneration for the scientific methods of the nineteenth century, Freud set himself apart from the scientific circles by his employment of techniques and theories that were marginal and even suspect to contemporary medicine.

That Freud was a physician *malgré lui même* has been stated so frequently by him and his biographers that little remains to be said about it now. More, however, needs to be said about the literary and social ideas which induced him to study for an admittedly undesired profession, and about the marginal techniques, which separated him from orthodox medicine at the outset of his psychiatric

career. The ideas are those of Goethe and Darwin and the techniques were those of, what was then called, "moral philosophy" and hypnotism.

In his autobiography Freud stated that never had he felt "any particular predilection for the career of a physician." But in the same paragraph he mentions the theories of Darwin and particularly "Goethe's beautiful essay on Nature . . . that decided [him] to become a medical student."[2] In view of the great impact of this essay—it had a decisive influence not only on Freud, but on a number of other nineteenth century scientists—I should like to quote some of its passages. This essay is now rarely read, but I hope that the few passages I shall quote will convey to you a little of the impact it had on Freud when he heard it for the first time.

Nature! We are surrounded and enveloped by her—unable to step out of her and unable to enter more deeply into her. Without being asked and without warning, she draws us into the round of her dance and drives onward with us until we are tired and fall from her arms.

She creates eternally new forms; what is has never yet been, what has been does not come again —everything is new and yet always the old.

She seems to have designed everything for individuality and does not care about the individuals. She always builds and always destroys, and her workshop is inaccessible.

Every one of her works has its own being, every one of her phenomena represents the most isolated concept, and nevertheless, all things come to One.

Even the most unnatural is nature, *even the dullest philistinism has something of her genius.* Whosoever does not see her everywhere does not see her anywhere rightly.

She enjoys illusion. Like the severest tyrant, she punishes him who destroys illusion in himself and in others. Whoever follows her with trust, him she presses like a child to her heart.

She hurls her creatures forth from nothingness and tells them not whence they come nor whither they go. They are just to run; the course *she* knows.

She provides needs because she loves movement. Miraculous that she attains all this movement with so little. Every need is a boon, quickly satisfied, quickly aroused. Each additional need is a new source of delight;

One obeys her laws even if one opposes them; one cooperates *with* her even if one wants to work *against* her.

Everything she gives she turns into a boon, for it is she who makes it indispensable. She tarries, so that one desire her; she hurries, lest one become satiated.

Her crown is love. Only through love does one come near her. She creates chasms between all beings, and everything strives to devour everything. She has isolated all things in order to draw all things together. With a few draughts from the chalice of love she compensates for a life of sorrow.

She has placed me in this world, and she will also lead me out of it. I entrust myself to her. She may deal with me. She will not hate her work. I did not speak of her. Now, whatever is true and whatever is false, everything was said by her. Everything is her fault, everything is her merit.[3]

We will never know how much of Freud's deep impression was conditioned by Goethe's authorship or by the ideas expressed in this prose-poem; for there is a good likelihood that the great poet was not actually its author, but that it stemmed from the pen of a Swiss clergyman, Georg Christoph Tobler, with whom Goethe had conducted a lengthy correspondence.[4] Goethe himself admitted not remembering its composition, but since it was so close to his own thoughts on nature, he treated it as his own. In analyzing it—as he did many of his other writings—he said: "One notices the inclination towards a kind of pantheism. For the thought is that an unfathomable, unconditional, humorously self-contradictory being underlies the phenomena of this world."[5]

With this statement, made in 1828, nearly fifty years after the composition of the essay on *Nature*, Goethe touched upon the widening dichotomy between religion and science. Later in the nineteenth century this dichotomy turned into a bitter struggle and led to the socio-intellectual upheaval that paved the way for the dynamic concepts in psychiatry. The spark for this struggle was, of course, set off by the appearance of Charles Darwin's *Origin of Species*. This work was published in 1859 and its entire first edition is said to have been sold on the

[2] Freud, Sigmund. *Autobiography*. Translated by James Strachey, New York, 1935.

[3] Goethe, Johann Wolfgang. *Die Natur*, ein Aphorismus. (about 1870).

[4] *Die Antike*, Bd. 15, 1939, p. 274 ff. See also Trunz (Hamburg Edition) Bd. 13, p. 45, 1955.

[5] Goethe an den Kanzler v. Müller, Weimar, Mai 24, 1828 von (vgl. Tagebuch von 23. März 1828) Eckermann betitelt "Erläuterung zu dem Aphoristischen Aufsatz Die 'Natur' " Hamburger Ausgabe. Bd. 13, p. 48 f.

day of publication. Subsequent editions and translations into many languages followed right away, and in the seventies, when Freud was in his teens, Darwin was surely the best known author of his day, and his works were among the most widely read of contemporary literature. Freud's reading of Darwin was by no means exceptional. Exceptional, however, was the extent to which Darwin's thoughts paved the way and corroborated Freud's findings.

The changes which were wrought by Darwin in regard to healthy and aberrant mental processes can best be appreciated after a brief glance at the earlier times when body and mind were sharply divided and held to be regulated by entirely different laws. The origin of the dichotomy between the healer of the body and that of the soul doubtless arose in the Middle Ages when physical illness became divorced from that of the mind. At that time medicine was at the low ebb of its art and unable to understand the mental patient. Odd behavior was attributed to witchery and possession by the devil and the patient was turned over to the clergy. When medicine rallied during the Renaissance, it had so completely forgotten its responsibility toward mental disease that the psychiatric patients continued their march to the halls of inquisition, or, if they were lucky, to the jails and "bedlams." The effort of individual physicians to relate the diseases of the mind to those of the body were ignored or derided, partly by the medical men themselves who felt powerless to deal with disturbed humans, and partly by the clergy who were reluctant to give up what they held to be the only tangible proof of the existence of inherent evil and deviltry. Even in the early nineteenth century when Pinel propounded the first tenets of modern psychiatry, the tradition of its divorce from medicine lingered on.

Actually, in Pinel's day the term "psychiatry" was not in existence, and even after it was coined in 1844 it was rarely used. The subject was referred to as psychology or, more frequently, as "moral philosophy," which was expressive of the strong ethical connotations applied to concepts of emotional and mental life. It was the scientific success of the evolutionary doctrine that dispelled the moral connotations of psychology. Since man was no longer considered to be a creature of God, his thoughts also ceased to be guided by inscrutable superhuman laws. And with the removal of the cloudy concept of morality the way was opened to the scientific investigation of man's mental processes.

Darwin's impact and later the great influence of Herbert Spencer on the subsequent psychological developments were broadened by Haeckel's biogenetic law of 1872, which states in essence that the development of an individual human embryo repeats in its maturation the life of the phylogenetic development. Stanley Hall and other psychologists elaborated this law even further by applying it to the psyche of man. Beyond that they hypothesized that the development of human mentality might be paralleled by the mental life of the individual. In their search for proof they studied the mentality of the child and of primitive man, and they were eventually able to demonstrate that the mind of the child, like that of the primitive, was simply an early stage of adult development.

The influence of these evolutionary theories on Freud's work can be detected easily. For psychoanalysis dealt with the same evolutionary problems, although in a somewhat reverse order. In the minds of the neurotic and the insane adults Freud and his disciples searched for vestiges of the childish mind and often found analogies between the reactions of their patients and those of primitive man. Indeed, in the preface to his third edition of *Three Contributions to the Theory of Sex* Freud himself refers to the biogenetic law, which is generally known as that of Ernst Haeckel, by stating that "ontogeny can simply be regarded as a repetition of phylogeny." In consequence he discovered in the behavior of adult neurotic patients an active persistence of the thought and feelings of their childhood. And in dreams he found an expression of desires and sensations of a primitive character in terms of the dreamer's infancy, and often in terms of the infancy of the human race.[6]

It is difficult to see Freud's work in its

[6] Bromberg, Walter. "Some Social Aspects of the History of Psychiatry," *Bulletin of the History of Medicine*, vol. 9, No. 2, 1942.

proper focus without a glance at the contemporary psychiatric developments, which are closely connected with the work of Griesinger, Meynert, and Kraepelin. In 1898 Theodor Ziehen, a philosopher and psychiatrist, described "psychology and psychotherapy as stepchildren of medicine." This was a considerable exaggeration, although the therapeutic aspect of psychiatry drew the considerably shorter end.

Psychiatry was, as I mentioned earlier, a young discipline, and it suffered from the urgent need to establish itself as a scientific branch of medicine. Even Pinel turned toward this end after his initial fortunate empiricism that led him to remove the chains from the insane and to replace their eternal incarceration with occupational therapy.

Pinel began his nosological studies so as to be able to segregate his patients for more efficient handling and treatment. But his classification of the insane into maniacs, melancholiacs, dements, and idiots did not stop with his own practical purposes. The general success of the Linnean system made classification per se scientifically desirable and it often appeared as though Pinel and particularly his successors rested content in classifying rather than treating their patients. Pinel's *Nosographie philosophique, ou la méthode de l'analyse appliqué a la medecine* (Paris 1798) became the authoritative work of a good part of the nineteenth century. It strove to determine the common elements in different cases of the same disease and led to subsequent attempts of correlating neural symptomatology and morphologic change. The few correct diagnoses which were occasionally made of specific pathologic processes were more frequently the result of good fortune than of any fundamental insight into disordered neural function. Not only was neurology far behind other fields of medicine in methods of diagnosis, but neurological disease complexes themselves were only poorly defined.

The one decisive nineteenth century figure in the formal development of neurology and neuropathology was Jean-Marie Charcot. He directed his efforts to the refinement and clarification of the clinical characteristics of the disease entities of his period, and, in the course of his studies, he was able to prove with finality that many conditions which previously had been considered entities were only symptoms. He abandoned classification according to dissociated symptoms and, instead, attempted to construct typical progressive symptom complexes and to correlate these with pathology.

In 1885, when Freud came to study with Charcot at the Sâlpetrière, French psychiatry had passed its zenith, although Charcot himself was then at the height of his career. In addition to his great neurological acumen, it was his use of hypnotism in the study of hysteria that attracted a great deal of attention. In contrast to Liébault and Bernheim of the Nancy School, Charcot regarded the hypnotic state as a neurotic condition, similar to, if not identical with hysteria; and there was a long-drawn battle between the school of the Sâlpetrière and that of Nancy as to the part played by suggestion. Unlike Liébault and Bernheim who used hypnotism for therapeutic purposes, Charcot believed that its only value lay in the diagnosis of hysteria.

Charcot's personality as well as his demonstration of male and female hysteria were profoundly influential on Freud's work. Freud's admiration continued undiminished even when he realized that Charcot's preoccupation with neuroses was rather superficial. In this connection Freud stated in his *Autobiography*: "Before leaving Paris I discussed with the great man a plan for a comparative study of hysterical and organic paralyses. I wished to establish the thesis that in hysteria paralyses and anesthesias of the various parts of the body are demarcated according to the popular idea of their limits and not according to anatomical facts. He agreed with this view, but it was easy to see that in reality he took no special interest in penetrating more deeply into the psychology of neuroses." It is interesting to see how closely this impression was borne out by Havelock Ellis' observation of the great neurologist: "For purely psychological investigation he had no liking and probably no aptitude. Anyone who was privileged to observe his methods of work at the Sâlpetrière will easily recall the great master's towering figure; the disdainful expression,

sometimes, even, it seemed, a little sour; the lofty bearing, which enthusiastic admirers called Napoleonic. The questions addressed to the patient were cold, distant, sometimes impatient. Charcot clearly had little faith in the value of any results so attained."[7]

If Charcot, who made such extensive and dramatic use of hypnotism, thought nothing of its therapeutic value, it is of little wonder that the majority of the European physicians and psychiatrists considered hypnotism in terms of Mesmerism, i.e., of unmitigated charlatanry. That Freud began his therapeutic career by hypnotic suggestion of hysterics stamped him immediately as aligned with pseudo-science. In the medical circles of Vienna slumbering memories of Mesmer's early successes came to life and the traditional hostility toward animal magnetism was extended to Freud's work. Freud's trip to Nancy to perfect himself in his hypnotic technique did not help matters, even though the school of Liébault and Bernheim had won out in its dispute with Charcot.

The medical world of the latter nineteenth century was firm in its repudiation of hypnotism. In his *Progress in Medicine* Dr. Galdston describes the period as that of inductive science, when practically every branch of biology and medicine underwent enormous development. Within the span of fifty years fall all the great achievements of bacteriology, the conquest of disease by means of vaccines, antitoxins, and communal sanitation. All of these triumphs were fruits of the research laboratories, where men studied the living organism and found their answers upon the slide of the microscope, within the chemist's retort and test tube, in the living tissues of the experimental animal and upon the autopsy table. They were impatient with any study that strayed outside of their fields, they were incapable of interest in any data that could not be counted, weighed, and measured with instruments. Hypnotism and suggestion were anathemas to the scientific spirit and mind of the nineteenth century, and so were many others of Freud's important concepts.

But these golden days of medicine, epitomized by the name of Virchow, simply did not include the total human being in its researches. The neglect of psychology indicates nothing other than that progress in scientific thinking was purchased at the price of letting one entire realm of nature—the mind—remain a reservation for religious and magical thinking. The scientist was an unconscious mystic in the mental sphere. Actually, despite the distance between Freud and Virchow, they had much in common. Freud investigated the mental world with the same scientific courage that Virchow applied to the physical world. That meant rebellion against the prevailing prejudices. It represents the same spirit of liberal thinking in science that in ethics proclaimed "the rights of man."[8] Yet, all this is easier to see in retrospect.

At the time when Franz Nissl voiced the optimistic hope of creating—literally—an "anatomy of Mental Disease," Freud began to elaborate his theories of the unconscious, which belonged, so far as the medical profession was concerned into the realm of purely speculative philosophy.

The physicians knew that Edward von Hartmann (1842–1906), the first to deal with the concept of the unconscious in a systematic metaphysical way, had not been a medical man, but a former army officer who had turned to the philosophy of Hagel, Schopenhauer, and Schelling. He had also familiarized himself with medical literature and had learned to understand the unconscious processes in the central nervous system, the reflexes, and many other aspects of bodily function. Von Hartmann placed not only the will—i.e., that which is illogical and irrational—into the realm of what he called unconscious, but he also attributed to it the imagination, which he considered logical and rational. These thoughts which von Hartmann himself described as the "Philosophy of the Unconscious" were ignored by the somatic psychiatrists when they were first propounded, and they were equally unacceptable in Freud's considerably elaborated version.

[7] Biographies of Havelock Ellis. By Isaac Goldberg, Boston, 1926 and H. Peterson, Boston 1928.

[8] Fenichel, Otto. "Freud's Place in the History of Science," *Psychoanalytic Quarterly*, 1946, v. 15, p. 282.

Hartmann's use of the word "philosophy" in relation to the unconscious was typical for the intellectual climate of Freud's period. There were then two schools that dealt with man's mental life. One of these devoted itself to the study of the psyche, so reminiscent of theological faith, that it is completely independent of the soma, or body, merely residing within it. The other school practically denied the existence of the psyche and attributed all psychological phenomena to organic, mechanistic causes. The first group made of psychiatry a branch of philosophy; the second group spent itself by searching feverishly and in vain for the cellular seat and evidence of neuroses.

It was Freud who cut through these controversies and diverging points of view. Psyche and soma he took to be two organically related aspects of the living organism, and he demonstrated that it was the psyche that could engender physical disease. However, the psyche does not function capriciously. Its operations in health and disease are determined by somatic factors and by such forces as emanate from or are applied to it by the social milieu. The latter Freud described as the ideals and taboos of the individual's realm.[9]

As was to be expected, neither the philosophical nor the somatic school of psychiatry was content with Freud's intermingling of their ideas, nor with his own thoughts and additions. Freud's teachings were antagonistic to both the rational and the moral points of view. At best he was ignored, at the worst vilified and attacked by his opponents. Freud himself wrote of this early period: "Psychoanalysis is my own creation; for ten years I was the only one occupied with it, and all the annoyance which this new subject caused among my contemporaries has been hurled upon my head in the form of criticism."

In 1917 Freud explained this antagonism as the result of man's wounded narcissistic self-esteem brought about by the discoveries of psychoanalysis; and he himself compared this narcissistic humiliation with two other similar humiliations which mankind has suffered as a result of scientific knowledge: the cosmological humiliation for which Copernicus was responsible when he proved that the earth is not the center of the universe, and the biological humiliation caused by Darwin when he showed that man is an animal like all other animals.

By now man has become resigned to the earth's insignificant place in the universe and to his close relationship to the animal world. But it is harder to accept the realization that, in Freud's words, "the ego is not master in its own house."

The London *Lancet* touches upon this question most poignantly and with a rather un-British exuberance: "Freud has been acclaimed another Darwin, and there are resemblances between the opposition he has met and that met by the pioneer of modern theories of evolution. Darwin wounded man's anthropocentric pride; Freud struck deeper and shook his egocentric beliefs. We are now recovering from the shock and realize that we have lived through another of those controversies that, as the discoveries arousing them, form landmarks in the history of science."[10]

In equating Freud's impact on society with that of Darwin, the London *Lancet* paid its highest tribute to the Austrian analyst. It must have been eminently satisfactory to Freud, who so readily acknowledged the role that Darwin had played in his own development. But it is significant that the world also acknowledged his affinity with the other genius, Goethe, who, like Darwin, had helped to shape Freud in his most formative years. In 1930, the city of Frankfurt awarded him the Goethe prize, one of the highest honors known within Germany. According to the rules covering the bestowal of the Goethe Prize, "It is to be given to the personality whose work has been generally recognized and whose creative efforts are worthy of being honored in memory of Goethe." Only once before had this prize been given to a physician, and this physician had been Albert Schweitzer. The wording of the citation that accompanied the awarding of the prize is significant in view of the earlier resistance against Freud's ideas in Germany and in view of the curtain that was to fall on that country only two years later. It

[9] Galdston, Iago. *Progress in Medicine*, New York, 1940, pp. 272–273.

[10] Vol. 230, May 2, 1936, p. 1016.

reads: "By a strictly scientific method and by the bold interpretation of the symbols created by the poets, Sigmund Freud found an access to the driving forces of the soul, and thus he created the possibility of recognizing the origin and structure of cultures and of curing various of their illnesses. Psychoanalysis stirred up and enriched not only the science of medicine, but also the world of ideas of the artists, the preachers, the historians and the educators. . . The Committee, after weighing carefully all arguments for and against, wishes by this honor to point the use of Freud's conceptual world as a pathway to a new affirmation of values cleansed of antiquated ideas."

Freud and Experimental Psychology

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My purpose is to review Sigmund Freud's relationship to general experimental psychology—to the kind of academic psychology that has grown up in our universities around the laboratories that were established toward the end of the nineteenth century. Offhand we think of these two kinds of psychology—the academic variety and Freud's psychodynamic variety—as being at opposite poles. General experimental psychology began with studies of sensory thresholds, reaction time, nonsense syllable memorizing, and it has remained pretty close to its origins, except that the first experiments were done on men and the later ones very frequently have been done on animals. Freud, on the other hand, dealt from the start with suffering, neurotic people, and though he was earlier trained in the laboratory, what he is remembered for, the reason for celebrating his centennial, does not classify as laboratory science. Polar opposites—like laughter and tears—often turn out to have much in common. I hope to show today that Freud and general experimental psychology are today closer than one might suppose.

American psychologists' early awareness of Freud

To the best of my knowledge, the first review of Freud's work to appear in an American psychological journal was one in the *American Journal of Psychology*, volume 10, 1898–99, under the editorship of G. Stanley Hall, E. C. Sanford, and E. B. Titchener.[1] The review was by Erwin W. Runkle, a Yale Ph.D. in 1893, and Professor of Psychology and Ethics for many years at Pennsylvania State College. He was an Honorary Fellow at Clark University in 1899, and one suspects that Stanley Hall gave him the *Studies on Hysteria* of Breuer and Freud to review. The studies had appeared in 1895. The review was quite favorable, and closed with the following words:

> The book will thus prove suggestive to those hidebound psychological thinkers who are overdogmatic in fixing the limits of the normal in conscious life, as well as those who view the abnormal as *ipso facto* of no direct value to the task which psychology has ever at hand, the systematic explanation of conscious experiences.

The fine hand of Stanley Hall is suggested by an unsigned notice in the same volume of a book on sexual motives by Gattel, in which the writer of the notice says: "The author, a nerve specialist in Berlin, has evidently been profoundly influenced by . . . Freud's recent brilliant studies on hysteria."

The reviewer, by the way, uses the word *libido* as though it is fully understandable without explanation for his readers in 1898–99. This led me to look further through this volume.

Without detracting at all from Freud's great contributions, I confess to some astonishment to find the amount of interest in topics we associate with Freud appearing

[1] It has since been called to my attention that William James reviewed some of the journal articles on hysteria in the *Psychological Review* in 1894. See Reference 2.

independently of Freud in this early volume of the *American Journal of Psychology*. There were reports on the psychological aspects of cures via Christian Science (Mary Baker Eddy's *Science and Health* being a "psychological" book of 1875, the year after Brentano and Wundt's classical volumes); reviews of Krafft-Ebing, Havelock Ellis; Durkheim's book on suicide; Clifford Howard's book on the phallic origin of religion (the reviewer accusing him of seeing phallic emblems symbolized everywhere and in everything); long articles on dreams, earliest childhood memories, Hall's study of anger. There is always something to be attributed to the *Zeitgeist* as well as to the great man!

Now a period of silence about psychoanalysis in the *American Journal of Psychology* for ten years, until Stanley Hall has invited Freud to lecture at Clark, and his lectures, buttressed by a great deal more of psychoanalysis, appear in volume 21, in 1910.

The psychological literature section in 1910 reviews a large number of papers by Freud and his pupils, and permits Hall (though the review is unsigned) to write an editorial about Freud at Clark, how impressed they all were as he spoke in German so quietly they could hardly hear him without gathering in close. Hall is so audacious that even in 1910 he says Freud is leaving Wundt and Kraepelin behind. William James, present at the lectures, said to Ernest Jones that the future belonged to Freud and his followers. James was in his last year, and his death notice appears in the same volume with Freud's lectures.

Academic psychology was treated to a heavy dose of psychoanalysis in this volume. Freud wrote on the origin and development of psychoanalysis; Ferenczi on the psychological analysis of dreams; Ernest Jones both on Freud's theory of dreams, and "The Oedipus complex as an explanation of Hamlet's mystery: a study in motive;" Jung on the association method.

Freud was thus launched, but the victory was not yet his.

The assimilation of Freud's ideas within American textbooks

The mechanisms of adjustment were the features of Freudian psychology earliest domesticated within American academic psychology (7). They now have a respectable place in our textbooks, regardless of the theoretical biases of our textbook writers.

Gates collected the mechanisms into a list in a textbook intended for the general student. The evolution of his chapter on mechanisms is itself instructive by showing how styles change in psychology. In his *Psychology for Students of Education* (1923) Gates called the chapter "The dynamic role of instincts in habit formation." In the first edition of his *Elementary Psychology* (1925) he changed the title to "The dynamic role of the dominant human urges in habit formation." Then in the next edition (1928) he used the contemporary sounding title: "Motivation and adjustment." The content of the chapter underwent only minor revisions with these changes in title. These widely used books did much to place the mechanisms on the tips of the tongues of psychology students and professors 30 years ago, for by that time the mechanisms were already part of the general equipment of psychology and not reserved for abnormal psychology or the clinic.

Some of the tendencies found in Gates's early treatments have persisted in more recent discussions of the mechanisms. For one thing, we took over the mechanisms when as a profession we were hostile to other aspects of psychoanalytic teaching. (This was the era of Knight Dunlap's *Mysticism, Freudianism and Scientific Psychology* (1920), and Joseph Jastrow's *The House that Freud Built* (1930).) As a consequence, we gave only halting recognition to the psychoanalytic origins of the mechanisms. Among the mechanisms in Gates's 1928 list, psychoanalytic writers originated introversion, identification, rationalization, projection, defense mechanisms, and compensation. Yet Gates's only mention of psychoanalysis was in some disparaging remarks about the "alleged adjustment by repression to the unconscious," an explanation of adjustment that he rejected as neither true nor useful.

Freud's distinction between manifest and latent content of dreams found its way into textbooks, but a careful study by Herma, Kris, and Shor (1943) (6) showed that scarcely anything else from the *Interpreta-*

tion of Dreams got reflected in them. They pointed out another interesting facet to Freud's influence: *acceptance* of Freudian ideas is usually *anonymous*, while rejection is *personalized*. This helps to account for the impression that academic psychologists are less open to Freudian ideas than in fact they are.

Freud and the behaviorists

John B. Watson announced his program of behaviorism in 1913, just a few years after Freud had been introduced to America. Here again appear to be some polar opposites: Watson denying consciousness altogether, and Freud not only dealing with conscious affect, but resting heavily on the study of the most subjective phenomena of all—dreams—and positing the importance of the unconscious. Yet there was an affinity between psychoanalysis and behaviorism from the very earliest years. I wish to call attention to some signs of this affinity, and to make some speculative comments about it.

A serious work by E. B. Holt appeared in 1915, with the title *The Freudian Wish and Its Place in Ethics*. Holt was what might be called a purposive behaviorist, to adopt the term that his student, Tolman, later used to describe his own behaviorism. He welcomed the purposiveness of Freud's system, going so far as to say: Freud ". . . has given us a key to the explanation of mind."

Watson was much impressed by Freud, though of course he could not accept Freud's words. We tend to remember men by the extreme things for which they stand, and so we think of Watson's conditioned reflex behaviorism and his extreme environmentalism (late in his career) as though they told the whole story about him. We do well to remember that Watson's effort was primarily to get rid of sterility in psychology, and he welcomed the heartiness of the Freudian approach. Watson's *Psychology from the Standpoint of a Behaviorist*, appearing in 1919, was one of the first general textbooks to devote a chapter to personality. Let me quote from this chapter:

> The development of many but not all of the blocked tendencies can be traced to childhood or adolescence, which is a period of strain and excitement. In childhood we often see the boy reacting to his mother in some respects like his father. The girl likewise becomes attached to her father and reacts to him as her mother would under certain circumstances. These tendencies from the standpoint of popular morality are perfectly "innocent." But as children grow older they learn from one or another source that such ways of reacting are either "wrong" or unusual, then the process of discarding and replacement is necessary. The replacement or substitution is often very imperfect. The apostle's saying "When we become men we put off childish things" was written before the days of modern psychology. We do not put them away—we replace them but they never for us lose their impulsive power. Parents who show excessive emotional reactions toward their children—over much fondling of them—often encourage such reactions and make normal substitution difficult (pp. 417–418).

This long passage illustrates what Herma, Kris, and Shor found: the positive contributions from Freud often go unlabelled. Here we have the pattern of psychosexual development and the Oedipal situation spelled out in the original behaviorist textbook. The language of consciousness and unconsciousness is not used; it is rather interesting how familiar the story is, even though told in Watson's vocabulary of habit and reaction.

A little aside here. The accidents of university finance and administration took Watson's animal laboratory away from him at Johns Hopkins, and he was left with no alternative but to work with the infants available in the hospital. By this stroke of fate he became something of an authority on childhood, and a later book of his, called *Psychological Care of Infant and Child* (1928) became very influential. Its main thesis was based on a misinterpretation of Freud. This misinterpretation we have already had hinted in the passage I read. Watson believed that the danger inherent in the Oedipal situation arose through the excessive emotional attachments of parents to their children, to their excessive fondling of them. The trouble is in the word "excessive," and in order to avoid excesses he introduced the "hands off" policy of child-rearing. This coincided with the pediatricians' recommendations of orderly scheduling, and for a time became canonized in the publications of the U. S. Children's Bureau. It is an odd coincidence that the

child-rearing practices that later Freudians, such as Ribble and Spock, have tried to correct, had themselves become established by one much impressed by Freud.

As we come closer to the present time we find more signs of congenial relationships between behaviorism and psychoanalysis. From the psychoanalytic side we find French (1933) writing on the relationship between psychoanalysis and the work of Pavlov (3). We find Dollard, a practicing analyst, affiliating himself with Neal Miller, an analyzed behaviorist, and they together writing a book on *Personality and Psychotherapy* (1950), which is a mixture of psychoanalytic insights translated into the terms of a behaviorist learning theory. An earlier illustration is G. V. Hamilton's *Objective Psychopathology* (1925). The Yale developments stem from a seminar in which Clark Hull, a leading behaviorist, sponsored regular discussions in which the behavioristically oriented psychologists and anthropologists learned psychoanalysis from John Dollard, Earl Zinn, and Erik Erikson. This began early in 1936. One result was the book by Dollard and others on *Frustration and Aggression* (1939), testing a Freudian hypothesis. Hull's rather friendly attitude toward psychoanalysis is found in a short article entitled: "Modern behaviorism and psychoanalysis," based on a lecture before the New York Academy of Sciences, and published in their transactions for 1939 (9).

Another main branch of behaviorism—that of Tolman—also carried on its friendliness to psychoanalysis. Psychologists in the Bay Region learned a good deal about psychoanalysis from Dr. Siegfried Bernfeld and Dr. Bernhard Berliner, who met informally with groups of psychologists and others, usually in the home of Dr. Ernst Wolff, during the late 1930's. Later on our education continued under Erik Erikson, Nevitt Sanford, and Else Frenkel-Brunswik. Tolman listened some to each of these, and met in frequent discussion with Dr. Kroeber, the anthropologist, who had himself practiced analysis for a term. We find frequent friendly references to Freud in Tolman's writings.

I could call the roll more fully. Guthrie, another behaviorist, isn't friendly to Freud, but that was because he is loyal to Janet. His behaviorism does not prevent his being "dynamic" in the sense in which Janet was.

Perhaps this is enough to show that the typical American academic psychology, if we can call behaviorism that, has been quite receptive to the teachings of Freud, with such emendations as the individuals prefer to make.

Why should there be this affinity between psychoanalysis and behaviorism? I shall venture some guesses.

1. *The scientific ideal of psychoanalysis and of behaviorism is very similar.* This ideal is that of a scientific world that would be pleasing to Helmholtz—a deterministic world, a world of energy-interchanges, a lawful world in which such principles as conservation of energy will hold.

2. *Both conceive psychology as a branch of biology.* Watson was grounded in neurology under Donaldson, just as Freud was trained under Meynert and Brücke and influenced by Darwin. The relative weight to be given to instincts, to brain processes and the like, is debatable within behaviorism just as it is within psychoanalysis, but the biological flavor persists within both traditions.

3. *Neither depends upon trained observers.* The subjectivity of reports by patients undergoing psychoanalysis is of a different order from that of a trained observer reporting his sensations and images. The report of the patient is a mere datum, to be studied and interpreted by the "investigator"; his words are data with the same status as his tics and mannerisms. They have no special virtue as reports of "direct experience." Hence behaviorism's quarrel with introspective psychology does not carry over to psychoanalysis.

4. *Both psychoanalysis and behaviorism are dynamic psychologies.* The concept of "drive" which for the behaviorist replaced the earlier "instinct" is not too different in its control of behavior from the "libido" of Freud. The flavor of the two is similar: behavior being irrationally triggered off by drive states and external stimuli related to these, earlier habits and affective responses persisting even when they are unconscious (to Watson, and also to Freud, unverbalized.)[2] The pro-

[2] For Freud's identification of the unconscious with the unverbalized, see Reference 4, pp. 133–34.

test of behaviorism in its early days was against a static and futile psychology; the philosophical issues were not paramount, and another dynamic psychology could receive a hearing.

Whether or not these four reasons are all defensible, the history of affinity between psychoanalysis and behaviorism is clear, and the polarity disappears, or is at least much attenuated, when you examine the scientific outlook of behaviorist and analyst.

Fields of experimental inquiry

Freud's influence has been felt in a number of fields of experimental inquiry. I wish to point out here that the problem is not whether Freud was right or wrong in what he said on these topics. That is not the important point, for we ought to go on from Freud in any case. If he was right, we ought to move on to further refinements of the truths that he sensed; if he was wrong, we ought to discover the right answers to the problems that he set. He will be left behind in either case, and in both cases we will have learned from him.

Here are a few topics as reminders:[3]

1. *Anxiety as a drive*, made familiar by Mowrer and Miller and many others.

2. *The control of word associations and word recognitions.* Originally brought to the fore by Jung and Kent-Rosanoff; more recently raised in other forms in a controversy over perceptual defense.

3. *Repression, forgetting, and recall.* Rapaport has given us a whole book on this material.

4. *Fixation and regression.*

5. *Aggression and its displacement.*

6. *Conflict theory.*

A fairly substantial literature has been accumulating on these and related topics. At the empirical level this is a useful interaction between psychoanalysis and experimental psychology.

One has the feeling, however, that a good deal of rich material from psychoanalysis has eluded the academic psychologist. This if true, might be either because of a certain obtuseness on his part, or because this richer material is too difficult to bring under experimental control.

1. *Learning as related to stages of development.* Psychoanalytic theory at many points suggests that the very young child is unusually susceptible to permanent influences, so that, for example, dreams throughout life may be influenced by these early learnings. We know from ethologists such as Eckhard Hess that 10 minutes of imprinting in an 18-hour-old duckling may last a lifetime. Birds whose songs are very adaptable in the first year may in some instance never again show changes in the songs learned, even though they live for many years hearing other songs, even within their own species. If this general significance of early learning is true, then it should enter general psychological theory, and the evidence for it specified.

We are beginning to get evidence on the adult consequences of early childhood experiences, evidence that bears indirectly on the principle that childhood is especially important for learning. Some of this evidence, based on the study of many different cultures, has been summarized by Whiting and Child (11). One of their predictions was that socialization anxiety established in early childhood should affect the interpretation of causes of illness in adult life. Even in our own culture, with the emphasis upon the germ theory of disease, we have many other interpretations of illness: "It must have been something I ate," "I've been working too hard," "Your sins finally catch up with you." And our remedies show something of the magical quality too: cure by rest, by liquids, by bland foods, by play, by suntanning, by religious observances. If a "scientific" culture like ours has all these magical residues, it is not too much to expect that primitive interpretations of illness might be related to deep-seated anxieties.

Whiting and Child found 46 cultures that they could rate adequately on oral socialization anxiety in childhood, and on interpretations of illness. Of 20 cultures high in producing oral anxiety in childhood, 17 used oral interpretations of illness. Correspondingly, of 19 cultures low in oral anxiety, 13

[3] For a development of these topics, with literature citations, see the chapter entitled "Freud's psychodynamics", Reference 8.

did not use oral explanations of illness. Not all findings were as clear as this, but it is impressive that they were able to find statistically significant differences at all.

Another important suggestion from psychoanalysis is that of childhood amnesia. In later life we appear unable to recall events that must surely have impressed us at the time, and about which we must have talked. Because these events become secondarily linked with repression sequences, they too are repressed. Memories that are recalled are often trivial and perhaps distorted, the so-called "screen-memories," probably serving some of the purposes of repression. This whole field has scarcely been touched by the academic psychologist, who ought to be able to explore it with children, and not depend upon the retrospective accounts of adult patients.

The latency period has been related to readiness for schooling when, after the Oedipal problems are temporarily solved, the ego is ready to become curious about the external world. After a few years adolescence produces a new threat, and learning may again be disrupted.

Most of the attempts to study the latency period by nonpsychoanalysts have raised serious doubts about the existence of such a period, although an interesting study by Friedman (5) gives a good deal of indirect support to the presence of a latency period.

2. *Obstacles to learning.* In the case material from child guidance clinics we find many illustrations of obstacles to learning based on the personal history of the learner. Pearson's recent book *Psychoanalysis and the Education of the Child* (1954) is useful for the studies of obstacles to learning to which he calls attention. The teacher as a parent-figure may arouse false expectations or reinstate continuing battles; some symbols used in teaching may be so freighted with personal meanings as to be defended against; conflicts over authority may result in nonreading or in spelling handicaps, for no subject matter is clothed with arbitrary authority so much as English spelling. We have case material on school phobias, showing how extreme some of these problems can become. There is, however, rather little material from the learning laboratory that shows a recognition of these matters so important for learning.

Thus it is clear that the study of learning has been enriched by topics drawn from Freud, but much remains to be done.

Conclusion

Because what I have been saying has related Freud's work to that of experimental psychology, let me close with a quotation from E. G. Boring, whose *History of Experimental Psychology* is a contemporary classic. Boring says:

> It was Freud who put the dynamic conception of psychology where psychologists could see it and take it. They took it, slowly and with hesitation, accepting some principles while rejecting many of the trimmings. It is not likely that the history of psychology can be written in the next three centuries without mention of Freud's name and still claim to be a general history of psychology (1).

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COMMENT

Science and the Language of History

Toward knowledge the path of progress is a path of consciousness. It is most quickly traversed by those with the most effective and most widely shared models of their own activities. In our generation these have been mathematicians and physicists. The logic of modern symbols and the logic of modern physics have facilitated in those disciplines advance at rates heretofore unknown.[1]

History, though one of the oldest disciplines, has only the paltriest representations of its own operations. Historiography has yet to do for history what modern logic has done for mathematics. In mathematics we have no difficulty in seeing that we have a symbol system, that we develop various symbolic interrelationships, that the "truth" of mathematics is its utility in predicting physical behavior.[2] We don't confuse mathematics with reality, we don't think there is only one correct set of relationships among the symbols, we don't think there is anything more to what we are doing than what we are doing. In history we have symbols, they are interrelated, their truth is their usefulness for maximizing our satisfactions. But there the parallel ends, for in history we often write as if truth were words—as if things were our symbols, as if then there were one correct arrangement of the symbols, as if arranging them were something more than psychic action. Most historians know they are not telling the truth, but still believe they are trying to.

Since historians aren't using language operationally, what are they doing? In detail, we don't know. And we won't know until we understand, operationally, the electronics of man as a receiver, storer, originator, and transmitter of signal energy. But at least we can say that apart from the retransmission of earlier signals—that is, apart from the quoting of contemporaries—what we see on the page is out of the historian, not out of the past.[3] And consequently we can say, too, that history as it has been written will be significant as behavior signals and in no other way. It will be significant for the uses to which we can put it in understanding the operation of the central nervous system, and more particularly, in some instances for the uses to which we can put it in understanding the symbolic interaction between two or more central nervous systems as one phase in the interaction between two or more organisms.

What obscures this putative truth is not only that some historians say they are telling the truth, and others that they are trying to, but that those few who are urbane enough to see that they are mythographers rarely say so, and probably don't understand that the significance of myth is intraneural and interpersonal action.[4] We have truth only when we have effective and imitable prediction. Since in histories we never have language used that way, we never have any truth. We have myth. A myth is several people making the same noises, and beyond this statement it is almost impossible to go without making myths about myths. We cannot, for example, say with any assurance of being extra-

[1] It was also by efficiency in abstraction that in Athens the Sophists and Socratics strode well to the fore of their contemporaries.

[2] Testing mathematical formulations for internal self-consistency is, of course, not testing them for truth, but for internal self-consistency. It is one way of developing symbolic interrelationships.

[3] One might also be able to establish certain correlations between the historian's expression and his experience, including of course his experience of symbols out of the past. For example, a statistical study might reveal a significant positive correlation between certain phoneme patterns in the words he read and the words he wrote, or correlations in the emotional, structural, or ideational dimensions of symbols read and symbols written.

[4] To this significance of myth there is, of course, one critically important, modern addition: the use of mythologic formulations as conceptual models in operation definition. See Reference 9.

mythically correct that a myth is a shared perception or something of the sort, because no operationally validated meanings have yet been established for the role of words in perception.[5] When two people say the same words, unless those words are operationally defined almost the only words we can say operationally about them are those that reveal their interrelationships, their modes of similarity. In short, given mythical language—for example, a history—as yet nearly all we can do with it operationally is to discuss it as language.[6] Of course we can speak of it mythically, and we can select for use those myths which we would say have the greatest likelihood of being translated into operational concepts. We can use a mythology that might be made into a conceptual model.[7]

The principal mythologies employed in Western history have been elaborated and reified metaphors of human behavior. Of these perhaps the most consequential have been the interest-analysis of the Sophists, the theology-philosophy of the Augustinians and Scholastics, the "humours" of early medicine, the character language of Humanism, the post-Christian hedonism of the Enlightenment,[8] and the psychiatry-social science of the present day.[9] In history we must still write mythologically, for our language is pre-scientific.[10] Since with myth habitual use is the narcotic creating the illusion of reality,[11] on today's historian there is a strong pressure toward using the mythology of psychiatry and social science. But it is far more to the point that among the available mythologies of human behavior no other was ever used operationally and scientifically, and without any doubt this is the most compelling reason for the contemporary historian to write in that language. And there are further, related reasons: the extensive elaboration of its interrelationships, desirable in any symbolic system; and its conformity with the principle of parsimony, recognized at least since Newton's day for a great desideratum methodologically.[12]

For extensiveness of symbolic elaboration the theology-philosophy of the Augustinians and Scholastics qualifies for consideration by contemporary historians, and currently perhaps familiarity with it is increasing.

[5] For a comprehensive discussion of perception, with language as a phase of it, see the papers of the 1949–50 Clinical Psychology Symposium held at the University of Texas, Reference 2.

[6] Chemistry in Robert Boyle's day is an analogue of history today. Epistemologically speaking histories are still what chemistries were then—sets of words sometimes fervently believed in, lacking operational significance, of importance only for the comfort they produce in the user's mind and for their effects in his interpersonal relationships. *The Sceptical Chymist* (1661) is the equivalent of a historiographical essay or a critical bibliographical essay today. And my discussion here is at least a collateral descendant of Newton's attack on "hypotheses." For the Newtonian position, see, for example, Reference 4, chapter VII, section 1(C). For an exposition of operational definition, see Reference 3.

[7] Every mythic item of language, every metaphor may be seen as potentially an incomplete conceptual model.

[8] John Gay, Helvetius, Bentham, *et al.* developed a comprehensive hedonistic mythology for human behavior, which with Bentham included a representation of man in his symbol-using role—Bentham's "theory of fictions"—that has been of great interest to C. K. Ogden, Kenneth Burke and other leaders in symbol theory.

[9] Complementarily, for a categorization of modes of using language in imaginative literature, see Reference 1.

[10] The application by historians of the chi-square test to medieval population data would be an exceptional, limiting instance. For other exceptions, in the social sciences generally, see References 7 and 8.

[11] This illusion is produced by using for both operational referents and no operational referents symbol patterns with the same formal characteristics. Habitual use tends to breakdown even in language-conscious people the capability that might have differentiated from sets of words whose similarities are wholly symbolic those which in extra-verbal significance are radically different; need, and anxiety, to have some way of dealing mentally and cooperatively with external reality accepts a purely imaginary, mythologic one, if no other is available; further, mythology provides the satisfactions of group sanction, since to use it is to manifest a primary identification trait of the group itself, and, very probably, using similar symbols is at still deeper levels a way of making similar people. Learning a mythology has been a large part of learning to talk for most human beings, and for intellectuals in all ages and cultures including our own, mythological manipulation has been a principal staple of commerce.

[12] Whitehead of course attributed to the Renaissance revival of interest in Plato the parentage of this sense of the desirability of conceptual simplicity. See Reference 10.

But, questions of internal coherence and distracting associations apart, the preponderant argument against the use of it in writing history is the absence of operational definition. It has been used exclusively for direct intra-neural satisfactions and in patterning interpersonal relationships; it has been used not at all in the formulation of conceptual models for scientific investigation. The use of a "humours" mythology was widespread for many centuries but is now irretrievably of the past; modern endocrinology alone has made it permanently superannuated even as a sketch for a conceptual model. The character language of Humanism still provides many, perhaps most, of the terms we use in everyday speech and writing about humans, but its fragmentation—its lack of internal coherence and its violation of the methodological rule of parsimony—probably obviated its use even when the new psychiatric and social science mythologies were growing rapidly during the century c. 1850 – c. 1950. Enlightenment hedonism[13] was very strong in internal coherence and in such formulations as Bentham's it reached something close to the imaginable limit of conformity with the law of parsimony. Bentham's "felicific calculus" has probably never been used as a scientific research tool, but it is not impossible that it may be of at least passing interest to investigators in the near future.[14] However, since the Utilitarians' day so much of the psychology of both personal and group behavior can be read as an extended commentary on the inadequacies of functional rationality as a central term for understanding human beings that anything short of a radical revision of the imaginative content of "pleasure" will leave hedonism an unlikely model for significant research investigation.

For the historian interested in a science of behavior, as well as for social scientists generally, it appears then that much the best hope for a conceptual model with which their work can be made part of a true science of man lies with the approach made explicit in *Behavioral Science*. And until such time as transitions from the mythologic to the scientific use of symbols are achieved, we can probably contribute most to bringing them into practice by continuing to use and develop the contemporary mythology of psychiatry and the social sciences.

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[13] The interest-analysis of Thucydides may be seen as a less coherent and less explicit version of Enlightenment hedonism. See Reference 5.

[14] One difficulty with the hedonic formulations is that not being harnessed to research experiment they have remained wholly verbal and incorrigibly circular; starting from within the Humanistic character language they—like their relative *homo economicus* of classical economics—turn up only as unaccountable dysfunctions secondary gain, masochism, etc. For the best recent statement of the hedonistic mythology, see Reference 6.

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ABSTRACTS OF CURRENT LITERATURE WITH INTERDISCIPLINARY IMPLICATIONS

Prepared by Members of the Staff of the Mental Health Research Institute

Campbell, Donald T. **Perception as substitute trial and error.** *Psychol. Rev.*, 1956, 63, 330–342.

Selective survival among random variations is taken as a general paradigm for instances of organismic fit to environment. Darwinian theory of natural selection applies the model to the fit between the inherited characteristics of organisms and the opportunities provided by their habitats. Trial-and-error doctrines apply the model to learned fit between organismic response and environment. Ashby and Pringle have independently noted the formal parallel between evolution and learning.

Attention is called to a third level of organismic fit to environment, in the adaptive responses employed in the flexible execution of well-learned habits. For blind organisms, the trial-and-error component in the carrying out of a habit may be obvious; but for organisms with distance receptors, the smoothly guided yet flexible character of the execution of learned responses seems quite out of keeping with the random variation required by the model. An effort is made to resolve this incongruity by characterizing perceptual processes as substitute trial-and-error, containing a search component which takes the place of blind overt motor movements. The notion is a more primitive one than that of "vicarious trial-and-error." It seems relevant for the empirical inconsistencies in the problems of "what is learned" and "insight." As related to servomechanism models, the notion is to be clearly distinguished from the simple negative-feedback regulators, like thermostat or governor, in which the feedback comes from the outcome of the primary effector. But a suggestive parallel is available in complex servosystems such as the radar-controlled guiding of ship or projectile, in which a blindly emitted beam is selectively reflected, and is used to substitute for a trial-and-error of ship movements or projectiles. (Author's summary)

Frankel, Stanley. **On the design of automata and the interpretation of cerebral behavior.** *Psychometrika*, 1955, 20, 149–162.

In principle it is possible to design automata to display any explicitly described behavior. The McCulloch-Pitts "neuron" is a convenient elementary component for the control mechanisms of automata. Previously described techniques permit the design of an automaton which would arbitrarily well simulate human behavior. The difficulty of producing such a design lies primarily in formulating an explicit description of the required behavior. The control mechanism of such an automaton would be of very great logical complexity. Its mode of operation probably would not resemble that of a human brain. The brain is more plausibly represented by stochastic models as proposed by Hebb. Such models can more easily be designed or understood by reason of lesser logical complexity. A method of computational investigation of the functioning of such stochastic models is described. Several extremely simple models have been investigated. One is shown to have properties suggestive of learning ability. (Author's summary)

Morris, Charles, and Jones, Lyle V. **Value scales and dimensions.** *J. abnorm. soc. Psychol.*, 1955, 51, 523–535.

The paper is a step toward developing a quantifiable comparative method in the empirical-descriptive approach to the study of human values.

The subject is presented with thirteen paragraphs, each describing a "way to live,"

which he is to grade on a seven-point scale of attractiveness. The following is a sample paragraph.

> Way 4. Life is something to be enjoyed—sensuously enjoyed with relish and abandonment. The aim in life should not be to control the course of the world or society or the lives of others, but to be open and receptive to things and persons, and to delight in them. Life is more a festival than a workshop or a school for moral discipline. To let oneself go, to let things and persons affect oneself, is more important than to do or to do good. Such enjoyment, however, requires that we be self-centered enough to be keenly aware of what is happening and free for new happenings. So one should avoid entanglements, should not be self-sacrificing; one should be alone a lot, should have time for meditation and awareness of oneself. Solitude and sociality are both necessary in the good life.

The affective content of the thirteen "ways" can be summarized thus:

1. Refinement, moderation, restraint; participation in social life to understand and preserve the best attainments of man.
2. Go it alone; avoid outward action; know self; be self-sufficient.
3. Sympathy, concern for others, restraint of self.
4. Abandonment; sensuous enjoyment of life; delight in people and things; solitude and sociality are both necessary.
5. Live outwardly, energetically, cooperatively—for group achievement and enjoyment.
6. Constant activity, striving for improved techniques to control nature and society.
7. Acceptance of something from all ways of life but giving exclusive allegiance to none.
8. Carefree, relaxed, secure enjoyment.
9. Quiet receptivity to nature yields a rich self.
10. Dignity, self-control, without retreat from the world.
11. Give up the world and develop the inner self.
12. Outward energetic action.
13. Let oneself be used; remain close to persons and to nature.

The subjects were male college students comprising five groups: United States 1945–52; India 1949–50; Japan 1949; China 1948; Norway 1952.

The method of the study involved first the development of an interval scale for each country. Guilford's method was used for this determination. Next the methods of factor analysis were employed, which yielded five major factors involving value orientations:

A. Social Restraint and Self-Control
B. Enjoyment in Action
C. Withdrawal and Self-Sufficiency
D. Receptivity and Sympathetic Concern
E. Self-Indulgence

The loadings of the factors from each of the 13 "ways" were determined separately from the United States data and from the India data. The loadings were roughly similar. Finally the profiles of the five "cultures" were graphed in accordance with the attitude (ranging from +1.5 to −1.0) associated with each factor. Again there is a rough similarity of the profiles based on United States and India analyses of the factor loadings. The results based on United States analysis are as follows (factors listed beginning with the most positive attitude):

United States:	B, A, E, D, C
India:	A, D, B, E, C
Japan:	A, B, D, C, E
China:	B, D, A, E, C
Norway	A, B, D, E, C

The existence of the derived interval scale allows also the comparison of the degrees of acceptance of the various factors in the cultures studied as well as the determination of the variances of the degree of acceptance (pointing possibly to heterogeneity or to conflict in the realm of values).

The authors conclude that the ratings of the 13 ways are scalable, permitting the use of appropriate statistics; that there is evidence for the existence of a value structure holding across cultures (as given in the results of the Indian and United States orthogonal solutions); that the result obtained by the combined use of the factor scores "has penetration and power and does not merely reproduce the obvious." (*AR*)

Rashevsky, N. **A neural mechanism for adjustment to optimal conditions, with possible reference to visual accommodation.** *Bull. math. Biophys.*, 1956, 18, 189–198.

The paper presupposes knowledge of N. Rashevsky's previous work on neuron excitation and neural pathways; most of it is summarized in his book *Mathematical Biophysics* (revised edition, University of Chicago Press, 1948); the main points can be found also in the introductory chapter of Rashevsky's *Mathematical Biology of Social Behavior* (University of Chicago Press, 1951).

The author indicates a possible neural mechanism with the following property: it adjusts a variable r to such a value as to *maximize* another variable C, where C itself is a function of r, and it is known that $C(r)$ has one (and only one) maximum. The author shows clearly that if we require C to be maximized this is a more difficult task (i.e., the neural network involved must be more complex) than if C were to be adjusted to a fixed value.

The meaning of C and r need not be specified, so that the proposed mechanism may have a great many different applications in various biological situations. For example, C may be a satisfaction function, and r a parameter on which that satisfaction depends. Rashevsky discusses visual accommodation as a special example; r then is the radius of the lens, C may be contrast, or size of retinal image, or amount of information which the retina receives from the object. The adjustment of C is not effected with absolute precision but only within narrow limits, the interval of uncertainty depending on the system's parameters. (*ET*)

Toda, Masanao. **Information-receiving behavior in man.** *Psychol. Rev.*, 1956, 63, 204–212.

The purpose of human communication is not always, as for the engineer, the sending of as much information as possible and the correct reception of the emitted signals; often, instead, the sender may not intend any transmission at all, or he may be trying to send incorrect messages. Thus, it may be necessary to look at human communication from the viewpoint of game theory—without, however, assuming complete rationality of the players. Also, the objective probabilities of chance moves may not be known to the receiver, and since the substituted estimated probabilities may vary between players, some theoretically strictly determined games may be no longer so.

Another problem for the human receiver is the problem of meaning. Basically it implies, that he must be able to extract several information contents (primary or secondary, as the semantic meaning) from a message by switching to the appropriate set of states and using the correct decoding procedures, which very often are not given to the receiver but have to be developed by him.

Not only in the context of game theory does the problem of subjective probabilities appear. The human observer typically has to deal with a multitude of ensembles, which may not even be stable over time, or which may be so limited in signal transmission that the receiver is unable to estimate transmission rates. Therefore the most meaningful measure for him is the amount of information intake. Yet the objective probabilities frequently will be unknown to the receiver or may even be unspecifiable in a rigorous way. Therefore, the subjective probability estimates of the receiver have to be used for the definition of amount of information intake. If the objective probabilities are given by the information content, the difference between the subjective and objective probabilities can be defined as the information gain after reception of the objective probabilities. If, as frequently occurs, the receiver obtains from several senders different messages about the same subject, acceptance of a probability distribution from an unreliable source will lead to information loss which is defined inversely. The best solution for this problem of information synthesis from different sources is to average the different probabilities for each state after weighing them according to their reliabilities. If the information contents from different sources are not more or less false but incomplete, the solution takes a slightly different form. (*FS*)

About the Authors

Theron Alexander, co-author of "Children in a Society under Stress," is Director of the Bay County Child Guidance Clinic, a community mental health clinic affiliated with the Bureau of Mental Health, in Panama City, Florida. He was born in Springfield, Tennessee, and received his Ph.D. from the University of Chicago. His interest in primitive cultures was stimulated when "I first saw natives in their canoes coming from dark green islands in the Coral Sea to greet our ship. Studying developmental cultural experience of primitive people seems to me to be an aid in understanding our own complex experience." At Chicago he was introduced to the multidisciplinary approach in the study of human behavior, and the value of this approach has been emphasized by his working in a community and in a clinical setting with people of other training backgrounds. Among other writings, Alexander has just published a pamphlet for parents entitled *About You and Yours*, which deals with family life. At present he is working on a book on child psychology, which is being written more from a clinician's point of view than previous books on the subject.

Robert Anderson, Associate Professor of Anthropology, at the University of Utah, was born in Bessemer, Michigan, and received his Ph.D. from the University of Michigan. In discussing his collaboration with Alexander, he writes "The Cheyenne TAT project was suggested by Alexander, who devised the modified test. From the beginning, I saw it as cooperation through a simple division of labor—I to describe the relevant cultural background, and he to analyze and interpret the test data I had collected incidental to my field work." Anderson is interested in the ethnography of the Plains Indian and Basin tribes marginal to the Plains, particularly socio-ceremonial organization. At present he is working on a monograph on the Northern Cheyenne, and contributions to a general work on the American Indian.

Henry W. Brosin ("The Primary Process and Psychoses," in the Symposium) is Professor and Chairman of the Department of Psychiatry at the University of Pittsburgh School of Medicine and Director of the Western Psychiatric Institute and Clinic. Born in Blackwood, Virginia, he received his medical degree from the University of Wisconsin School of Medicine. He has been trained and practices as a psychoanalyst. He was a Colonel in the Medical Corp of the U. S. Army from 1941–1946, and was awarded the Legion of Merit. Brosin is widely known for his participation in numerous organizations, including the Group for the Advancement of Psychiatry, Committee on Army Education, National Research Council, Social Science Research Council, the National Advisory Council of the U. S. Public Health Service, and the American Board of Neurology and Psychiatry. His present research interests center about psychoanalytic research methods and research in psychosomatic problems as well as psychodynamics of lobotomized patients, linguistic-kinesic analysis, and sensory deprivation. His publications range broadly over psychiatry and behavioral science, numbering over a hundred. During the past year he has been a Fellow at the Center for Advanced Study in the Behavioral Sciences.

Michael R. A. Chance ("The Role of Convulsions in Behavior"), was born in Birmingham, England, and is currently Lecturer in Experimental Pharmacology at the University of Birmingham. His writings to date fall into three main categories: 1. The pharmacology of the mammalian brain and related studies in animal behavior, 2. studies on the social behavior of primates, and 3. philosophical articles. He states that having lived through and participated in the social upheavals of the '30s and '40s in Europe, "I have come on reflection to realise that these stem from the absence of validated knowledge of our own behaviour. The foundation of such knowledge lies clearly in the biology of such behaviour, and in biology certain hypotheses have already sufficient validation to act as such guides—this does not imply that they are easy to use—e.g. evolutionary theory and the adaptation of the organism to its environment. Applied as heuristic weapons, biological axioms are very potent tools of discovery; since they occupy such a fundamental status, all meaningful explanations should ultimately connect with and be a further test of these axioms—if our aim is to construct a com-

prehensive scientific explanation of our behaviour".

P. G. Herbst, author of "Situation Dynamics and the Theory of Behavior Systems", is currently research psychologist at the Tavistock Institute of Human Relations in London. Born in Görlitz, Germany, he attended the University of Melbourne, later the University of Pennsylvania, and at the present time is completing some work at the University College in London. He states that the basic ideas for his article go back to 1948, when he first came across the work of Kurt Lewin. "Not being able to see how within a psychological frame of reference one could distinguish events located inside the person from those located in his psychological environment, I explored some of the implications of a 'behavioural field' construct in which tension is regarded as a function of structure rather than segregated as a separate system." The following year he worked as a research assistant with the Community and Social Tensions Project at the Melbourne University, which provided an opportunity for translating problems of theory into problems of method. At the University of Pennsylvania, while working on a community study project, he was able to follow up some of the ideas which had emerged from this earlier study. At present Herbst is engaged in the development of methods for studying the relation between social, technological, and economic structure of human organizations. His publications include articles in the field of family relationships, labor turnover, and the structure and functioning of an autonomous working group.

Ernest R. Hilgard, author of "Freud and Experimental Psychology" (Symposium on Freud), is currently a Fellow at the Center for Advanced Study in the Behavioral Sciences, having been granted a leave of absence for one year from his duties as Professor of Psychology and Education at Stanford University, where until recently he has been Dean of the Graduate School. He received a B.S. degree in chemical engineering from the University of Illinois and a Ph.D. in psychology from Yale. He states that his interest in the possibility of bringing psychoanalytic theory into the orbit of general psychology began in the early 1930's "when I was given the task of abstracting the *British Journal of Medical Psychology* for *Psychological Abstracts*. The articles then appearing were both fascinating and a little repelling. The interest began to take more concrete form in informal seminars between psychologists and others conducted by a thoughtful psychoanalyst, Siegfried Bernfeld, in San Francisco. Having since served on the National Advisory Mental Health Council and on the advisory committee on emotional growth and mental health of the Ford Foundation, I feel that a great deal remains to be done to place some very germinal ideas on a sounder scientific footing." Dr. Hilgard's publications include four books: *Conditioning and Learning* (with D. G. Marquis), *Theories of Learning*, *Psychoanalysis as science* (with E. Pumpian-Mindlin and L. S. Kubie), and *Introduction to Psychology*, in addition to various journal articles.

Eric H. Lenneberg ("A Probabilistic Approach to Language Learning"), born in Duesseldorf, Germany, is currently Russell Sage postdoctoral fellow and special auditor in the Division of Medical Sciences, Harvard University, having received his Ph.D. in Social Psychology and Linguistics. His interest at present is centered on the development of a formal, nonspecific theory of generalization and concept formation. He plans to compare experimental and observational data obtained from normal American children with various kinds of handicapped children as well as normal children with radically different cultural and linguistic background. He is co-author of a monograph entitled *The Language of Experience* (Indiana University Publications in Anthropology and Linguistics, Memoir 13). The article in this issue is one of a series of studies on the subject.

Ilza Veith, author of "Freud's Place in the History of Medicine" (Symposium on Freud) is Assistant Professor in the History of Medicine at the University of Chicago. She was born in Ludwigshafen am Rhein, Germany, and attended medical schools at the Universities of Geneva and Vienna. She received a Ph.D. in the history of medicine from the Johns Hopkins University, the first in this country to be awarded such a degree. Since 1947 she has been consultant in Far-Eastern Medicine to the Armed Forces Medical Library, and for a two year period was editor of *Medicine and Biology* at the University of Chicago Press. She is the author of three books, *The Yellow Emperor's Classic of Internal Medicine*, *Medicine at the University of Chicago*, and *Perspectives in Physiology*, and numerous articles dealing with various aspects of the history of medicine.

Henry D. von Witzleben, one of the contributors to the Symposium on Freud, "History, Psychiatry, and the Behavioral Sciences," is Director of Professional Education at the Veterans Administration Hospital, Palo Alto, California, and Assistant Clinical Professor of Psychiatry at Stanford. He was born in Oldenburg, Germany, and attended medical school at the Universities of Bonn and Freiburg; his psychoanalytic training was received in Chicago. His prime interests center about the training of psychiatric residents, analytic psychotherapy with schizophrenics, and the history of psychiatric ideas. This latter interest is the theme of a book which he is now preparing, the purpose of which is to clarify certain concepts of, and relations between, psychiatry, history, and behavioral sciences. This, he feels, is necessary before it can be shown how and why psychiatric ideas are closely connected with specific cultures and civilizations (their myths, religion, art, etc.) from ancient Greece to the 20th century.

An Acknowledgment

Through inadvertence we neglected to acknowledge our indebtedness to Professor Paul Lazarsfeld for some of the material and references in the introduction of our article, "The Logical Nature of an Action Scheme." Specifically, the references to the Wurzburg School, G. Radbruch, the term "action-language," and the reference to "Handlung as a frame of reference" must have entered our thinking because one of us had the opportunity to read Professor Lazarsfeld's unpublished manuscript, "Some Historical Notes on the Empirical Study of Action." At the time of writing neither of us recalled having actually made use of that material in any specific form. Since publication, this inadvertence (psychiatrists have a whole literature on this and allied phenomena) has been called to our attention, and we hope by this note to give credit where credit is due for the influence of this material on our introductory remarks.

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EVOLUTION VS. "REVOLUTION" IN PSYCHOTHERAPY: A BIODYNAMIC INTEGRATION[1]

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No one but an osteologist can be truly entranced by a human skeleton, whereas, a physician can accurately imagine that skeleton clothed in the flesh and blood of a man and can then picture that man exquisitely alive and creative. It is with this reliance on the supplemental imagery, knowledge, and experience of the readers of this journal that the skeletal outline of psychotherapy is offered—with the added recognition that both the structure of skeletons and our knowledge of them also change over the ages. Nevertheless, such changes occur not by violent revolutions and counter-revolutions, but by an orderly *evolution* along pre-set and significant patterns.

It is a strange phenomenon, in itself worthy of group dynamic study, that in the past when anyone planned to write a paper for a psychoanalytic meeting, his thinking was quite likely to follow a prescribed mold. Whether or not he quoted Freud directly, he would almost always pay reverent tribute to some fragment of "classical" theory, cite a few exegetics by previous writers and then proceed, with labile logic and in *largamente* lingo, to offer yet another "Contribution to the Modification of our Understanding of the Unconscious Psychodynamics of. . . ." Unfortunately such contributions (including, in years past, my own) eventually became so sterile and soporific that many behavioral scientists ceased to read psychoanalytic books and journals. It is one of the purposes of our Academy to repair these deeply regrettable schisms of communication among psychoanalysts and their colleagues in related disciplines.

As a step in this direction, the present paper will deal with psychotherapy from a broadly comparative and biodynamic rather than a monothetic standpoint, and in operationally meaningful rather than technically illusive language. Accordingly, instead of attempting to recount the wealth of relatively recent contributions, including Zilboorg's and Moreno's concepts of successive "revolutions" in the field, I propose to derive certain broad generalizations from a wide range of data marshalled into the three heuristic categories of all behavioral sciences: I. the *historical*, II. the controlled *experimental*, and III. in the case of the biological sciences, the "bedside" or *clinical* (Gr. *klinikos*, bed).

I. THE LESSONS OF HISTORY

A. Organismic integration

Herbert Muller, in his *Uses of the Past* trenchantly remarked that "Our age is notorious for its want of [any deeper] sense of history than the minutes of the last meeting." In psychiatry particularly we seem to prefer the trivial writings of yesterday to the deathless lessons of a half billion years of biologic evolution. Nor is a preceding cosmogeny tenfold as long irrelevant, since in modern metaphysics the distinction between "material" events and the "vital processes" which are their extension and apprehension can hardly be maintained. For example, in his masterly *Science and*

[1] Read as an address of the President-elect at the Midwinter Meeting of the Academy of Psychoanalysis, New York, December 9, 1956.

the Modern World the physicist-mathematician-philosopher Alfred N. Whitehead thus expresses the thesis that there is an unbroken organismic line of past, present, and future "events" and "locations," *from nuclear particles to human behavior*:

> In being aware of the bodily experience, we must thereby be aware of aspects of the whole spatio-temporal world as mirrored within the bodily life. . . . In a certain sense, everything is everywhere at all times . . . every spatio-temporal standpoint mirrors the world (p. 93). The event is a necessary link in the pattern of transmission, by which the character of every event receives some modification from the character of every other event (p. 154).

Or, as John Donne put it more poetically: "No man is an Island, intire of itselfe . . . never send to know for whom the bell tolls: it tolls for thee!"

This apprehension of universal interrelatedness was probably immanent in the first vital stirrings of the primal Algonkian slime; certainly, the most primitive living things still apply the Golden Rule in a way that gives a wry twist of meaning to the phrase "descent of man." For example, according to Gerard (5, 6), the cells of the lowly *myxamoeba Dictyostelium discorideum* ordinarily live a life of rugged individualism; however, should shortages of food or water develop, *a few individuals form the nucleus of a colony and then die to constitute a stalk upon which the others may sporulate and survive*. As Bonner (1) also points out in his *Cells and Societies*, at only a slightly higher level of cellular aggregation—say that of the *Physalia pelagica*, no one can say whether the creature we call a "Portuguese Man-of-War" is merely a group of independent cells, a loosely assembled colony of individuals, an "animal" in the biologic sense, or a fully organized and functioning society. Simpson in his *Meaning of Evolution* (16) comments:

> I know that some students of the subject deny that any animals below man . . . are "aware" or "perceive" anything whatsoever; for the present purpose, this does not matter in the least; all animals *act* as if they had awareness and perception—even the ameba does (p. 21). The basic fact that all phyla of animals have survived since their origin bothered Sigmund Freud, who could not see why all ancient forms have not yielded to a death wish (p. 19). Nor, as Huxley remarked, does ascribing evolution to an *élan vital* [cf. libido, Thanatos] explain the history of life [any more] than . . . an *élan locomotif* explains the operation of a steam engine (p. 131). Struggle is not necessarily or even usually of the essence. Precisely the opposite; selections in favor of harmonious or cooperative group association is certainly common (p. 96).

Here, then, are the first manifestations of the fundamental biodynamic principles of *interaction* and *aggregation*—names for the "forces" that assemble atoms into men and men into societies. In this sense even Mesmer was intuitively correct; human beings, like iron filings, are drawn to their kind by a sort of "animal magnetism;" further, as Saul (15) has pointed out, *aggression* too literally means "to move toward."

B. Anthropologic integration

Available records indicate that the earliest creatures that can be termed *homo sapiens* were already well advanced in respect to communion and collaboration, in that they lived not only in family groups but in cohesive clans organized for mutual welfare. That such organizations strove to preserve the most intimate of all biodynamic relationships—child to mother and man to woman—is indicated by the figurines of steatopygous females ("paleolithic Venuses") whose huge breasts, bellies, and buttocks epitomized the acme of maternity and sexuality. Concurrently there appeared man's projection of wishful imagery into the future, as represented in the cave paintings near Lascaux in France or Altamera in Spain. Gombrich (7) in his *The Story of Art* regards these scenographs and those of even earlier Acheuleian cultures as displaying nothing less than "formalized artistic perfection." However, it is not their esthetic striving that makes them uniquely human: crabs too, have decorated their shells since Cambrian times, bower birds and trade rats beautify their nests and many other creatures cheer themselves and the universe with color and song. But the Mousterian sketches of tribal hunts and customs depict not only *individual skills* and *aggregate actions*, but also a third principle fundamental to human behavior and therefore to psychotherapy: namely, that if a yearning hope

is but eloquently and insistently enough expressed, some transcendently powerful Parental Surrogate will answer the plea and grant satisfaction to the supplicant. As Movius (13) in "Archaeology and Earliest Art" analyzes the evidence, their protoreligious function is the most likely explanation of why these paintings were placed in temple-like alcoves, covered over like palimpsests and surrounded by other objects of primitive worship. To quote Movius, "the stenciled or negative hands on the cave walls reflect possession or power . . . the darts or arrows piercing certain animals signify the casting of a spell on the game." To recapitulate, then, fifty or more thousand years ago man already derived essential comfort from three faiths utilized by all therapy today:

First, man's perennial trust in his capacity to control his milieu through various technologies, from chipped flints then to space satellites now.[2]

Second, a wishful reliance on the collaboration of his fellow-man, and

Third, an indissoluble assumption of power to employ one or another form of Supreme Magic to serve his own purposes.

I have elsewhere explored the derivations and implications of these Ur-fantasies in considerable detail (cf. my *Practice of Dynamic Psychiatry*), and we shall again scrutinize their involvement in the history of psychotherapy in the last section of this paper. Here, however, let us examine what further generalizations we can derive from the contemporaneous behavior of man's fellow creatures.

II. EXPERIMENTAL APPROACHES TO INTEGRATIVE THERAPY

One of the corollaries of all of these Ur-defenses is that man tries desperately to retain as long as possible the conviction that he is unique as well as paramount in this universe. I refer not alone to his doctrines of geocentricity, special creation, the immortality of the human soul, anthropomorphic gods robed and bearded like hired priests and prophets, or other such theologic narcissisms. What is of greater present import is the subtler drag of man's egotism on supposedly objective research and derived science. For example, long after Friedrich Wohler synthesized urea from ammonium acetate in 1828 many chemists refused to abandon Berzelius' "vitalist" distinctions between "inorganic" and organic compounds—a resistance recently rearoused by Stanley's demonstration that a crystallizable protein like the tobacco virus can also be a living thing. So also, nearly a century after the publication of Darwin's *Origin of Species* many presumably serious students of the field are loath to concede that man's anatomic and physiologic characteristics evolved in a continuous biologic line. It is therefore understandable that to defend a last bastion many men, including some psychiatrists, still contend that human behavior is, and presumably always has been, either completely unique, or else so much more complex than that of even the "closest" of man's fellow-creatures that a comparative-evolutionary approach is interestingly futile.

Admittedly, all analogies are in some sense misleading, but in this connection the following may be more helpful than trite: It is perfectly true that one cannot learn the labyrinthine anatomy of the human central nervous system solely by comparing it with the primitive neuraxis of say, an Amphioxus, since the human system, especially its telencephalic portions, is far more intricately evolved. And yet the dialectically opposite statement is equally true: one cannot understand the basic organization of the human system *without* studying that of the Amphioxus, since only then can the fundamental structure (or "plan") common to all vertebrates in the evolutionary sense be grasped. So also, one cannot analyze human conduct merely by studying mice in mazes, cats in cages, or monkeys in ataraxic hazes; nevertheless, such relatively objective and controllable studies can lead to the clarification of

[2] This is not uniquely human: wasps use pebbles to tamp their tunnels; finches use cactus spines to dig insects; ant colonies raise food crops and tend aphids for their "milk"; and beavers engineer dams and build submarine cities. Moreover, like their intransigeant human cousins, they continue to do so no matter how often their activities lead to frustration or catastrophe.

certain principles fundamental to all behavior. We may here restate four basic Principles of Biodynamics thus derived[3] as follows:

Motivation: All behavior is motivated by physiologic needs: survival, procreation and, possibly also, esthetic creativity—in various configurations of contingency and urgency.

Adaptation: Every organism reacts not to an absolute "reality," but to its own interpretations of its milieu in terms of its past experiences and uniquely developed capacities and patterns.

Displacement: Whenever object-directed activities are frustrated by external obstacles, the organism tries either (*a*) different methods to reach the same goal or, (*b*) other partially or wholly substitutable objectives.

Conflict: However, when two or more urgent motivations are in sufficiently serious opposition so that the adaptive patterns attendant to each are mutually exclusive, the organism experiences a mounting internal tension ("anxiety") while its somatic and motor behavior becomes persistently ambivalent, ineffectively substitutive, and poorly adaptive (i.e., "neurotic") and/or progressively disorganized, regressive, and bizarrely symbolic ("psychotic").

As another corollary to these principles it becomes evident that the milieu for optimum activity must be sufficiently challenging to maintain vitality and creativity, but not so frustrating as to cause excessively deviant conduct. However, if neurotic or psychotic behavior is induced by disruptive conflicts, it also follows that the objectives of therapy are to resolve these to the point of restoring the most favorable degrees of adaptation compatible with retained initiative. It is therefore of interest that hundreds of experiments by Pavlov, Gantt, Yerkes, Jacobson, Wikler, the author and their respective associates (cf. Masserman, *Principles of Dynamic Psychiatry*, 1946), have demonstrated that the following procedures, analogous to those long empirically employed in human, are effective in relieving "experimentally induced" neuroses and psychoses in animals:

1. A temporary or permanent change to a milieu less likely either directly or by association to induce frustration or conflict.

2. The biologic satiation of one or more of the conflicting needs.

3. Arbitrarily forcing the animal to surmount external obstacles or internal deterrents to necessary activities and thus find new patterns of satisfaction.

4. Placing the animal among others of its kind so that their "normal" behavior can furnish examples of favorable adaptation.

5. Utilizing "sets" or "attitudes" derived from previous intra- and interspecies experiences as a basis for effective "transference" relationships which may then be utilized to aid the animal to re-explore ("analyze") its past and current reactions to its milieu and thus resolve its conflicts.

6. The measured use of various drugs that facilitate this process of reorientation by producing temporary amnesias for previously traumatic events and thus dulling derived associations and anxieties.

7. The production when necessary for similar purposes of more persistent impairments of central nervous function either by metabolic paralysis (anoxia, insulin hypoglycemia) or by direct tissue diaschisis (electroshock or cerebral surgery).

The clinical significance of these therapeutic techniques has been developed elsewhere and need not be repeated here. However, it may be noted in this context that, like their clinical counterparts, each of these methods offers almost limitless opportunities and variations for influencing behavior, and each is hedged about by handicaps and contraindications. Nor, on deeper analysis, can they be as clearly separated from one another as would appear from the simplified listing above. For example, a monkey subjected to experimentally induced conflicts between hunger and an apparently innate "symbolic" fear of a toy rubber snake will develop serious "psychosomatic" dysfunctions and become progressively more inhibited, compulsive,

[3] Cf. my *Behavior and Neurosis*, 1943.

phobic, pervasively suspicious (paranoid), and unrealistically stereotyped (delusional) in its behavior. Method 5, say, can then be used in therapy: i.e., the animal's confidence in some mentor is rewon and it is then gently but progressively retrained to resolve its conflicts and once again to seek and secure food. However, this necessarily also implies (*a*) a "psychologically" changed milieu, (*b*) the concurrent alleviation of an impasse between hunger and supposed self-preservation, (*c*) an externally facilitated solution of the problem, and (*d*) a change in behavior pattern exemplified by the mentor—hence all five "methods of psychotherapy" are involved simultaneously. But the therapeutic contingencies may be rendered even more complex by "constitutional" or other biodynamic factors: for example, Method 5 can be employed only in spider monkeys or in relatively young macaques, since only these will respond by becoming dependent on the experimenter's ministrations; in contrast, a neurotic vervet is comforted most effectively by the presence of a mate, whereas the group-oriented adult cebus is helped mainly by re-participation, perhaps necessarily in an altered role, in the life of the cebus colony. Nor are the therapeutic effects of drugs, electroshock, or lobotomy to be regarded as specific, since their actions too, vary not only with the particular drug or procedure and the physiologic status of the organism, but also with its concurrent motivations and adaptations, its life experiences before and after the therapy in question and, for that matter, the beliefs, attitudes, and derived actions of the experimenter-observer. As stated in the beginning of this section, then, laboratory data may help to clarify the biodynamic principles of behavior relevant to effective therapy, but at the same time their proper interpretation also reveals how intimately interrelated at all levels all treatment procedures are.

III. THE CLINICAL EVOLUTION OF INTEGRATIVE PSYCHOTHERAPY

It is a whimsical thought that although the word *experience* means "to pierce from outside," so little of the wisdom of seven millennia of empiric psychotherapy seems to have penetrated into clearly formulated consciousness. But this too, is not as strange as it seems—man fears to scrutinize too closely and disillusively that which he "loves": i.e., that upon which he hopes to rely; on the contrary, he prefers to clothe it in enhancing mystery. But whether or not our Idols of the Cave, though conceived in glory, are made of clay, they indeed have power over those that believe in them. Let us therefore here examine the Idols of Psychotherapy in a more enlightened Spencerian manner, provided we also avoid sacrilege by acknowledging their undoubted potency under whatever names, ancient or modern, their high priests worship and invoke them.

In the introductory section we saw how prehistoric man, in his struggles for survival, relied on a Triune Core of Faiths. We recall these as: first, the extension of his own powers through personal inventiveness and skill; second, the development of wishful trust in and collaboration with his fellow-creatures (including domesticated plants, animals, and humans) and third, the deification of omnipotent Beings who were then beseeched, bribed, or bedevilled into serving man in larger matters of immortality and cosmic control.

It must be understood that the Ur-fantasies here enumerated were never made explicit any more than, in only a slightly different sense, were the guarded techniques of artisans, the shibboleths of select societies, or the magic names of Baal or Yahveh. Nevertheless, each of these primary faiths engendered so many interlocking traditions and customs that only the briefest indication of their nature is here possible.

Thus our great-grandfather 250± in Akkad, when troubled by frustrations and conflicts, was first required to attempt to solve his problems by his own skills and ingenuities. If these failed he could summon a group of his fellows in a designated Place of Succor for advice and assistance. If lay efforts were insufficient, a general physician or a specialist could be engaged

at a standard fee, and with pre-set penalties for failure.[4] Should all human efforts prove inadequate, final recourse was had to the Temple, where accommodating female attendants were commissioned to comfort him in the name of Mother Ishtar, and where a priest commanded various supernatural minions to help the needful votary with both his earthly and heavenly affairs. In Eighteenth-Dynasty Egypt, thirteen centuries before Christ, temple therapy also included such regressively-cathected activities as prolonged ablutions, specially prepared foods, confessional prayers for forgiveness, and a trance to induce dreams for interpretation by the priest. But even more than this was available: by following the magic ritual of the Book of the Dead, Seth, Osiris, and all lesser deities could be compelled by the knowledgeable Egyptian not only to remove his frustrations and slay his mundane enemies, but also to banish all obstacles to immortal suzerainty in the Land of Ra—especially if the magical formulae ended, as they do now, by an invocation of the supreme god Amen! We may well ask: could any system of therapy provide better escapes from harsh realities, more satisfying recourse to temporarily regressive physical satisfactions, greater alleviations of past and present anxieties, and more glowing promises of everlasting power and glory? By all accounts, the Egyptians of the middle millennia before Christ were indeed a happy people.

Nor was this empirically acquired wisdom of their forebears altogether lost on the iconoclastic Greeks. Their beautifully situated Asklepiad sanatoria (literally, health resorts) were also made ancillary to Hellenic temples where, through the medium of esthetic Apollonian or ecstatic Dionysian ritual, priest and priestess offered religious as well as physical retreat and satisfaction to the devout and avid participant. But the Greek genius for development and detail now added to the essential Ur-processes the following maneuvers, each included in one way or another in their medico-religious therapy:

Faith in Self-help: On the physical side, calisthenics, athletics, and other practices to develop the Body Beautiful and Triumphant. But physical strength was also to be multiplied manifold by Superior Knowledge, whether of the philosophical systems of the Ionic philosophers, the historical suppositions of a Thucydides, the cosmic harmonies of a Pythagoras, the encyclopedic "nature"—observations of an Aristotle, or the supposed certainties of Platonic absolutism. Nor, for that matter did the Greeks fail to admire and utilize the technologic-scientific application of an Archimedes, who two centuries before Christ, could confidently proclaim "Give me but a place to stand, and with my levers I can move the world!" Greeks of the Athenian stripe were a confident breed, and thoroughly exploited the reassuring maxims "Knowledge is Power," and "Knowledge sets men free!"

Faith in Fellow-men: This second Ur-fantasy was also utilized in a number of ways, divisible for purposes of discussion (though indivisible in dynamic essence) as follows:

Interpersonal: Depending on his conscious or unconscious needs, the patient could explore and satisfy vis-à-vis his physician-priest the following ("transference") expectations and attitudes:

(*a*) As a kindly and devoted parental or avuncular surrogate, who personally welcomed the troubled prodigal back to a haven of security and comfort. This was reinforced at the Sanatoria by intensive attention to bodily satisfactions through special diets (dietotherapy), baths (balneotherapy), massages (physiotherapy), and other services reminiscent of the attentions given a cherished infant (Gr. *therapos*, service).

(*b*) As a learned and experienced guide and leader, whose counsels and systems for more restrained and balanced, and therefore healthier and happier modes of life could be followed on rational and empirical grounds (e.g., as in the various Stoic schools).

(*c*) The patient could also regard his mentor as more personally interested, willing to listen to the supplicant's complaints ("present illness") explore their

[4] Such procedures and practices were later specified and regulated in the Code of Hammurabi (c. 1250 B.C.).

relationship through personal reminiscences to the patient's past experiences (history), derived meanings and values (semantics) and patterns of goal-directed action (operational analysis); the formulated "verbal" understandings so derived could then be applied to foster not only more satisfying, but more lasting and useful adaptations (operational "insight"). Socrates required his students to work through their own verbal perplexities; Plato understood the conative nature of dreams and symbols, Aristophanes in his delightful comedy "The Clouds" pictured the distraught Strepsiades lying on a couch and trying to acquire understanding through fantasy and free association; and Soranus records the cure of a case of "hysteria" by resolving an underlying sexual conflict by unequivocal action.

(*d*) Finally, the transference could also take the form of trusting submission to the healer as physician, with avid acceptance of the efficacy of his quasi-scientific, quasi-mystical remedies. And the armamentarium available to the Hellenic physician included not only the vast and mysterious pharmacopoeias of all the lands from Cathay to the Gates of Hercules, but also a vast variety of surgical and other manipulations (such as the Egyptian practice of trephining the skull and incising the cortex or, as described by Pliny the Elder, making electric eels discharge through the head) available for the treatment of the weak or ailing. We read of Hippocrates' condemnations of the ignorance and superstition inherent in many of these "false remedies" but this alone proves how widely practiced, then as now, they must have been.

Group Relationships: In this category of therapy too the Greeks anticipated us by nearly two and a half millennia. Moreover, they apparently were wise enough to recognize—where we sometimes forget—that although reversion to various forms of passive interpersonal dependence may be desirable in the first stages of therapy, excessive anaclisis may encourage paralyzing regression, and must therefore be followed as soon as practicable by measures designed to foster personal redevelopment and social rehabilitation. These may at first require a favorable and protected environment where special techniques and expert supervision are available, but the objective continually must be to re-deploy the patient's motivations, methods, and skills for returning to creative interpersonal living. According to medical historians (cf. Garrison, Zilboorg, Fulton, Riese, *et al.*) the following would be only a partial list of the media for group communication and activity used in and out of sanatoria from Greek times to the present:

Music, giving opportunity for feelings of conjoint rhythm and harmony, esthetic expression, group belongingness, and many other advantageous experiences (see my "Music as a Tool of Delightful Delusion" [10] and other writings in this field).

Calisthenics and Dancing, with similar possibilities of group catharsis and communion.

Competitive Athletics, with opportunities not only for the joy of action but through nondestructive competition for public recognition and reward.

Dramatics: Here the poetic psychiatrist (and there can be no other) may again ask: What writings better explore or epitomize basic human relationship than the plays of Euripedes, Aeschylus, or Aristophanes? And what productions can offer either the witness or participant more varied identifications, vivid experiences, or vicariously tragic or comic solutions of his own interpersonal problems? The Greeks cherished and utilized these dramas for their deep human empathies and meanings, endlessly varied their themes and were deeply involved as players, chorus, or affectively moved audience. They thus explored in essence the basic individual-group interactions utilized in the psychodramas of Moreno and other forms of group therapy.

Institutional Service: This offered a transition between a passive dependence on the sanatorium to an eventual recognition of an obligation to return to a place in the community maintainable only through responsibility and service for the common good.

The Ur-defense of Transcendent Magic: As to this last belief even the civilized Greeks demanded that Socrates pay the ultimate

penalty for threatening the most cherished of all human defenses: a trust in the existence of celestial Beings who, though all-wise and all-powerful, were nevertheless gullible enough to serve man in exchange for flattery, cajolery or, at most, a little bribery. To capitalize on this ultimate faith the Asklepiad sanatoria, like many hospitals today, were ancillary to one or another religious cult which added the following powerful factors to therapy:

A "divinely revealed" doctrine in which all believers could feel an exclusively self-elevating bond of fellowship.

A reassuring ritual which, through its origin in human needs and through millennia of empiric refinement, included such exquisitely gratifying procedures as:

The symbolic eating and drinking of the parent-god's body in the forms of mystically potentiating food and wine (as exemplified in the ancient worship of Melitta and Mithra).

The temple hymns, sung and played in the simple, repetitive, rhythmic cadences of a mother's lullaby—and often resulting in the hypnotic trances of the "temple sleep." This could be varied with Dionysian orgies to be doubly enjoyed, since they also honored one's permissive and accommodating gods.

The "anointing" or "laying on of hands" to cure an injured bodily part—a direct reminiscence of soothing parental care of an injured child. And, if necessary, the temple could make available occasional or even permanent cloistered retreats or "sanctuaries" from earthly stresses and problems.

The corresponding emphasis on the *spiritual*, a concept as fundamental to life as is the neonate's first breath or *spiritus*. Every human is variously *inspired*, acquires an *esprit de corps*, becomes *dispirited*, and finally *expires* so that his immutable *spirit* can begin life anew. And here, too, the physician-priest functions in knowing the Spiritual World, or purveying professed contrition and remorse to the Spirits of our Fathers, and of requiring only a gratifyingly small penance with which to avoid the horrors of eternal punishment.

And finally, the priest mediates the supreme promise of all religious—or, for that matter, of all "scientific" systems: the conquest, through life eternal, of man's most grim and implacable enemy of humankind: death itself.

It is tempting to take dialectically opposite attitudes to the foregoing account of the evolution of the essentials of psychotherapy. One extreme would be to regard it as demonstrating that all was known and practiced to perfection in a Golden Age long past, and that we have simply deteriorated into greater confusion and awkwardness since then. Opposite views might be (*a*) to regard the descriptions above solely as pseudo-historical fantasies parallel to wishful screen-memories, (*b*) to accept them only as approximations of wisdoms that were not really possessed at the time, much as the atomic theory was only vaguely anticipated by Democritus, or evolution by Anaximander, or (*c*) to assert that until recently men knew only "intuitively" what they were doing and applied it only sporadically and incompletely. Actually the last commentary would still be applicable to most of our psychotherapy, whereas the truth would lie somewhere closer to the first statement than to the strictures under *a* and *b*. In fact, though admittedly there is no record that all the practices mentioned were carried on to full potentiality at Ur, Memphis, Cos, or any other ancient citadel of healing, it is not altogether misleading to point out that no method of therapy in the intervening ages has utilized any *essentially* new principles or combination of techniques. Let us list a few of the seemingly diverse therapies that have remained effective through the ages because of this very fact:

Narcissistic Self-Assertions: Since men continue to equate bodily strength with personal, sexual, and social dominance, purveyors of perennial physical panaceas, from the fakirs or yoga chimeras in ancient India to the fakers of yogurt cheese today, will always have a dedicated following. Closely related are the Body-worship Cults from Sparta to Bernarr Macfadden—cults so firmly entrenched under slogans such as "A strong mind in a strong body" that the most sedentary superintendent will still

insist that his gymnasium is an essential part of his "mental hospital."

Also in the general category of narcissistic preoccupations, though less physically strenuous, are the various Orders of Adulation of the Supreme Self. These range from the blandly narcissistic "Every day I am getting better and better" of the late Emil Coué, through the pseudo-social, pseudo-religious pap peddled by the Dale Carnegies or Norman Vincent Peales, to the existentialisms of Heidigger, Husserl, Sartre, and related prophets of rationalized and rarified solipsism. And here too, may be placed two other significant movements which have much in common, despite their contrasting scientific goals; i.e., one is justly condemned as an ignorant fad, whereas the other, because of its sincere aspirations toward objectivity, critical self-examinations and vectorial follow-up research as to the results of treatment, has been avidly accepted by equally sincere therapists in various universities and "counseling centers." I refer first to the "dianetics" of E. Ron Hubbard which operated on the belief that any person, under the direction of an "operator" with only a minimum of knowledge and a few hours of training, can in a session or two of recalling "engrams of association," not only "clear" his past, present, and future of all obstacles and perplexities but emerge personally impregnable and capable of "operating" on others. The second system is the so-called "nondirective" and "client-centered therapy," which, with similar assumptions as to therapeutic time and training required, also postulates that the client's "Self," if merely given an opportunity to "grow" through "Self-expression" in the presence of a "counselor" who acts only as a compliant observer, will spread its own "inner strengths" and likewise emerge transcendent and triumphant. Both of these understandably popular cults necessarily term themselves "permissive," "warm," "self-determinative," "research-minded," and "scientific," and both must deny communal fantasies and symbolic interpersonal relationships between patient and therapist. But since narcissism, however euphemistically formulated, here merges into a potent *folie à deux*, these and similar perennially recurrent movements may be placed in a transitional position between the solipsistic and the interpersonal-manipulative therapies to be considered in the next section.

Interpersonal Therapies: These are, of course, infinite in variety, but a few leading examples may be mentioned under related dynamic categories as follows:

Maternal: Everyone who has once been a cherished child will thereafter welcome contrectation in various forms with kindly or powerful parental surrogates, whether priest, king, masseur, physiotherapist, or physician. Greatrakes "stroaked diseases of the Flesh" out of supplicating thousands in post-Cromwellian times just as chiropractors do today, and troubled human beings will always like to lie a-couched and be tended to manually as well as verbally. And when even deeper regression is invited by soothingly reiterative inducements to "relax and sleep" under maternally protective control, hypnotists exult in a mystic power and tend to forget Bernheim's maxim: "It is a wise hypnotist who knows who is hypnotizing whom."

Medico-thaumaturgic: Here the psychiatrist can employ not only the physician's armamentarium against bodily pain and diseases, but also specially developed methods for altering cerebral function either temporarily or lastingly by metabolic changes (CO_2, insulin), pharmacologic influences (barbiturates, "ataraxics")[5], physical diachisis (EST) or surgical incisions (lobotomy, thalamotomy). However, all of these

[5] The predictably insistent demand for "miracle drugs" to assuage psychosomatic sorrows was met successively by ancient extracts of precious stones, dragon's teeth, unicorn horn, or Galen's Samian Clay, medievally by powdered martyr's blood or the secret nostrums of university Professors of Physic, and more recently by glorified antibiotics, hormones, and "tranquilizers." As may also be expected, each new panacea is proclaimed and for a time defended by its devotees with a fervor formerly reserved for the manifestations of divine healing. In contrast, neurotic animals in our laboratory not directly or indirectly affected by such publicized enthusiasms respond much more favorably to the demonstrable pharmacologic actions of the barbiturates, alcohol, morphine and bromides than to nearly all of the supposedly thaumaturgic ataractics being foisted upon and accepted by an avid public.

inevitably carry or acquire a rich overlay of wishful belief, and when such fantasies of omniscience or of power over life and health (*sanatos*) are too intimately joined by the psychiatrist, the quasi-delusional interchanges thus constituted may produce highly charged favorable or unfavorable results.

"Psychoanalysis": Despite the rigidity of theory, teaching, and technique professed by some proponents who forget its dynamic evolution, psychoanalysis, if finally regarded as a deeply searching and individualized method of studying, formulating, and altering human behavior, can constitute the most inclusive and effective combination of narcissistic-supportive, interpersonal-transactive, and even religio-philosophic approaches to therapy. In effect, it develops all the categories outlined above as follows: From the personal standpoint, it affords a more detailed and dynamic re-examination and re-evaluation of determinative life experiences, symbol formations, and derived patterns of defense or goal-achievement. In parameters of interpersonal re-exploration, it presents rich opportunities for controlled "transference"[6] and "counter-transference" interactions with the analyst as confessor, rival, expiator, erotic object, judge, pedagogue, protector, and many other roles leading optimally to that of a maturely accepted friend. Finally, group-reorientative analytic experiences may become generalized and meaningful far beyond the analyst's office, and thus lead to more adaptive, stable, and creative familial, occupational, and other social readjustments. True, psychoanalysis can also be prostituted into an escapist, sterile cult for both patient and analyst, but this is unfortunately true of all methods.

Group Methods: I have discussed the principal theories and techniques of the group therapies elsewhere, (cf. my *Practice*) and have noted their historic role in total therapy above. Whether formally so designated or not, they can foster and guide an essential final series of steps in which the patient extends whatever insights and skill he has acquired to optimal familial, marital, occupational, and other social and cultural transactions. Without such consistent operational results all therapy, whatever the theoretical claims made for it, is only delusive or ephemeral.

Magico-Transcendent: In an article published in the *International Journal of Psychoanalysis* (9), I ventured the predictions, based on an exploratory analysis of the Triune Faiths of Man, that during the post-war period of atomic fears and political,

[6] "Transference" is another term that any writer who respects clarity and specificity of meaning must now hesitate to use. Freud employed it originally to connote the experientially derived attitudes which "from the beginning" color the patient's evaluation of the analyst, and which he "works through" in the therapeutic process. However, other writers (e.g., Sterba, Strachey, Glover, Klein, Alexander, Zetzel, *et al.*) have used the term variously and variably to mean interpersonal projections developed either *in* or *out* of the analysis proper, either *confined* to the analyst or *generalized* to others, either *influenced* or relatively *independent* of the therapist's methods or "counter-transference" and, most redundantly involved of all, either "narcissistic," "ego-syntonic" or "Super-ego-cathected," in shifting *normal*, *"normally neurotic"* or *"analytically-neurotic transference"* contexts. Moreover, a currently popular cult (e.g., Brill, Reider, Gitelson, *et al.*) contends that what peculiarly distinguishes psychoanalysis from (sic) psychotherapy is the formation of an emotionally disruptive "transference neurosis" as the most necessary part of the treatment. Freud himself (cf. his "Autobiography" and "Analysis Terminable and Interminable") strongly implied that the development of excessively regressive, aggressive, erotized or other neurotic transference relationships was an "artifact" of poor therapeutic technique; but then Freud, like other leaders before him, lost his status as a teacher and examplar long before he was canonized. Parenthetically, the peculiar doctrine that neurotic patients can be helped by analytic therapy only if the latter makes them temporarily or even permanently more so is whimsically reminiscent of Garrison's quotation of Teodorico Borgogni, who wrote six centuries ago:

"For it is not necessary . . . as many disciples teach and as modern surgeons profess, that [laudable] pus be [deliberately] generated in wounds. No error can be greater than this. Such a practice is indeed to hinder nature, to prolong the disease, and to prevent the conglutination and consolidation of the wound."

Needless to say, Teodorico was bitterly attacked for differing with the wisdom of current authorities and for his consequent ignorance of the best surgical technique of the day. For that matter, the desirability of "laudable pus" as a medium of healing was vigorously maintained long after Pasteur, Koch, and Lister in their time also condemned it as an "artifact of poor technique."

social, and moral disorganization there would arise various worldwide movements among which the following would be most prominent:

Cults of the Glorified Self—in philosophy, ethics, art, and psychology.

Formation of Cohesive Defensive-Aggressive Groups by supposedly strong mutual interests (such as occupation, age, nationality, language, etc.) with the corresponding adulation of glorified leaders by each such group. And, less overtly but perhaps most deeply significant of all,

The increased influence of organized mysticisms, especially expressed in the renewed appeal and power of various antirationalistic and religious institutions.

It is the relationship of this last phenomenon to psychotherapy with which we are here finally concerned. As but one example, the Christian Science Church, despite the supposed "materialism" of the past decade, has grown tremendously in appeal and power precisely because it combines an astonishing number of the potent therapeutic techniques already discussed under each of the Ur-fantasies dealt with above. In review, each Christian Scientist is aggrandized by assumed knowledge not only of the one true religion (Christianity) but of all relevant Science; each member is personally vouchsafed the canonized presence of a most energetic and provident Church mother—Mary Baker Eddy; the faith of each is daily renewed by her sacrosanct book—the *Science and Health*, as exclusively and interminably quoted by "readers" in the church services; each member is offered warm acceptance and intimate communion with a remarkably cohesive group of fellow-believers; and each is granted influence over other mortals through a Power of Healing not only by direct contact but by the quasidivine magic of "absent treatment." Such appeals are multiple and universal, and their potency is reflected in the fact that, however much we deplore its tragic consequences in individual cases, it is nevertheless a fact that Christian Science, with over 3000 highly active and proselytizing branches and societies in 46 countries, every year gives some measure of refuge and comfort to many more millions of people than will be directly helped by all psychiatrists in the next half century. Concurrently "pastoral counseling" in other denominations has become increasingly important, will continue to grow in general acceptance and influence, and may also tend to become more narrowly cultist, dogmatic, and mystic as man's vaunted sciences threaten to suffocate rather than succor him. Without belaboring the point, it becomes evident that the sophisticated therapist truly devoted to his calling will not only accept man's mystic yearnings as an essential determinant of human behavior, but will utilize them as among his most effective media of therapy.[7]

OPERATIONAL INTEGRATION

Clinically speaking, then, the essentials of psychotherapy are the following:

The maintenance of the scientific prestige, ethical integrity, and social influence of the psychiatric and allied professions and of the individual therapist. Differences of honest opinion, as in any mystico-scientific field, are acceptable; extreme claims, public polemics, and transparent exhibitionisms diminish our status and thereby our capacity to help.

The warm, unashamedly personal acceptance of each patient not as another "interesting case" in support of some pre-

[7] In this connection it is significant to note the rapid growth in membership and influence of the National Academy of Religion and Mental Health headed by two psychoanalysts, K. Appel and L. Bartemeier. In a single year the Academy has found acceptance as advisor to various departments of pastoral psychiatry at Loyola, Yeshiva and other universities, and has been granted nearly a half million dollars for the purpose by the National Institute of Mental Health.

In the meantime an apparently irresistible gratification of all of the Ur-defenses (technologic, social, and theophilic) is being offered in the "Dial-a-Prayer" service by which, for the price of a telephone call, any supplicant can hear a 30-second prayer recorded on a magnetic drum by a minister of his selection. A sample from Manhattan's Fifth Avenue Church is this: "My grace is sufficient for you, for my strength is perfected. When we . . . call upon God for help, His divine power flows into our lines and turns defeat into victory". The YMCA in Los Angeles already answers 10,000 calls for its "Dial-for-Inspiration" daily, and the Catholic Order of Our Lady of the Bell is endeavoring to make its own offerings nationwide. (*Time*, Dec. 10, 1956)

conceived theory, or as an object of inexorable analysis, or even a grist for another therapeutic mill, but as a hurt, frightened, and troubled human being seeking relief and guidance. These should be accorded him in the following way:

The skillful utilization of every medical means available to relieve bodily pain and dysfunction, or to suspend undesirable activities (at a minimum risk of permanent addiction or defect) until better ones can be established.

The tactful and sympathetic exploration of: the nature of the patient's motivations, the origins of his conscious and unconscious symbolic representations and value-systems and the objectives of his characteristic patterns of behavior in relation to their interlocking advantages and disadvantages in the present and probable future. This exploration may be formal, minute, and detailed (including "free associations," screen memories, dream material and so on), or it may be confined to a careful and perceptive analysis of interview, psychological test, or other clinical data. In either case, the first objective will be *to help the patient recognize that his previous patterns of behavior were neither as necessary nor as advantageous as he had implicitly assumed them to be.* But to leave the patient here would be to place him in a new quandary: his former ways are no longer even delusionally effective, but he has not as yet learned new and better ones to take their place. It is therefore equally necessary *to utilize optimal transference situations and other therapeutic opportunities to impart the second essential portion of the dual dicta of insight: namely that new patterns of conduct are really preferable*—not alone because they are legal, moral, or "mature," but because in the long run they will result in greater over-all satisfactions for all concerned. Such reorientations, which must be achieved without letting the patient become too regressive and dependent in the "anaclitic" phases of transference, may be rendered more effective by combinations of the following techniques.

Re-education of the patient by the therapist through reason, demonstration and, implicitly or explicitly, personal example.

Utilization of every available system of progressive social participation through enlightened self-interest, whether in supervised "milieu" or "group therapy" or as indirectly arranged with the patient's family, friends, employer, or social group. Here too, the deep influences of the patient's religious, political, or other loyalties, instead of being attacked, may be used to their best advantage. Indeed, it is the eventual efficacy of all of these re-adjustments that determines whether or not the patient will be re-accepted as a happy and useful member of his society—and this in turn will spell the success or failure of the therapist.

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THE DRUG PLACEBO—ITS PSYCHODYNAMIC AND CONDITIONAL REFLEX ACTION[1]

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The placebo, that is pseudo-medicine, has for centuries been used by physicians as medication on pragmatic grounds—"it helped patients." With the appearance of scientific foundations under medical practice questions about the placebo became pressing. How does it work? On what does it work? On whom does it work? In drug research the placebo, of course, plays the indispensable part of the "control." Many questions, however, arise concerning the nature and efficacy of such controls which cannot be answered without extensive research in which both physiological and psychological factors must be taken into account. In this paper the questions are posed and discussed in the light of recent experimental findings.

In the field of drug studies relating to the treatment of psychiatric disorders, sophistry and rationalization still continue to play too important a role. This is not deliberate but results from the environmental restrictions found in the usual clinical setting. Compromise seems to become the only recourse of the investigator if he, at least, is to begin an experimental approach to the problems confronting him.

The devices and techniques to which the investigator resorts by way of compromise fall into many categories. The approach he most commonly adopts is to employ a large number of patients in the hope that from such a study will come at least a trend. If a positive trend is indicated, attempts are made to check it against controls. This takes the course of trying to match patients, adapting a routine procedure, and manipulating the environment in such a way that whatever extraneous influences the patients are exposed to, will be common to them all. In evaluating the results, the investigator prefers rating scales to intuitive judgments, subjects any numerical quantities to statistical analysis, and uses placebos or the double blind approach in the administration of the drug being investigated. It is with the last factor that this study is concerned.

The use of the placebo confronts the investigator with two problems: (*a*) the effect of the placebo itself and (*b*) the reaction of the subject to a nonspecific stimulus. Too often vital considerations which complicate an analysis of drug reactivity are left unanswered: namely, how can a placebo produce a certain specific effect in one individual, an antithetical reaction in a second, and no reaction at all in the third.

It was the need for clarification regarding the placebo effect which initiated this review of the latest information available on the subject. Soon after the initiation of the project it was apparent to the reviewer that while the use of placebo is not new (for tradition and the common suggestibility of man argue for the antiquity of its use), the wide variability of its effect and

[1] It is with a marked feeling of appreciation that acknowledgment is made of the fruitful discussions on placebo reactivity which have been held with Dr. George F. Sutherland, Director of Psychiatric Education and Training of the Department of Mental Hygiene of the State of Maryland, Mr. Thomas Hanlon of the Research Department of Spring Grove State Hospital, Dr. T. Glyne Williams, Clinical Director of Spring Grove State Hospital, and Dr. Isadore Tuerk, Superintendent of Spring Grove State Hospital.

the factors influencing this variability remain relatively unexplored areas. As a consequence, even though there were numerous reports on placebo effect in the literature, the review turned out to be an unsatisfying venture. Most of these reports consisted merely in the citing of such an effect, with little or no additional information being offered to account for the underlying psychological and physiological variables involved. Not only are the relationships between these variables and degree of placebo reactivity a clouded issue, but also the problem of the duration of reactivity and the extent of its cyclical arousal are equally undefined. It became apparent to the reviewer that there is no centralized formulation of information on the subject or even a general bibliographical source from which this information could indirectly be obtained. In a recent paper Beecher (2) points out how Pepper ten years previously stated: "Apparently there has never been a paper published discussing (primarily) the important subject of the placebo." It is hoped that the present paper will help to meet this bibliographical need and also present a clearer and more concise picture of the current status of placebo research in experimental psychiatry.

This review is not immediately concerned with the theoretical considerations underlying the placebo effect, but rather with the insights and conclusions of experimental investigations. Of course, in any investigation of the placebo effect, one is immediately confronted with the limitations of our knowledge regarding the interrelationships between personality structure, the field of awareness, and the influence of such a dominant factor as that of suggestibility. Eysenck (6), in a review of the literature on the problem of suggestibility from the standpoint of tests which supposedly measure this trait, states that there is little evidence of a significant correlation between it and hysteria. This observation seems to be contrary to the general impression that many psychiatrists have regarding the suggestibility of the patient suffering from hysterical symptoms.

With the above paradoxical concept serving only as a hint to the complexity of the forces underlying placebo effect, it was felt that the most meaningful and practical approach of this review would be to provide current investigators with an awareness of some of the areas of this problem which have already been surveyed and of those areas which most fruitfully might be explored. It was also felt that some idea might be given regarding the type of experimental method most applicable for future research.

The nature and experimental use of placebo

The present extent of the use of the placebo is perhaps best indicated by an editorial appearing in a recent issue of the *British Medical Journal* (4), which states ". . . a bottle of medicine is given as a placebo in about 40% of the patients seen in general practice." Much of the modern scientific knowledge on the therapeutic action of the placebo is the result of controlled experimentation, the most comprehensive of which are the investigations of Beecher and his co-workers. In an analytical survey of the placebo effect, Beecher (2) presents the results of fifteen studies (7 of his own and 8 related studies chosen at random) involving 1,082 patients. He finds that placebos have an average significant effectiveness of 35.2 $\pm$ 2.2%. The studies selected by Beecher involve treatment of such conditions as severe postoperative wound pain, cough, drug-induced mood changes, pain from angina pectoris, headache, seasickness, anxiety and tension, experimental cough, and common cold. In this survey Beecher calls attention to the toxic and other subjective side effects of placebos noting such symptoms as dry mouth (9%), nausea (10%), sensation of heaviness (18%), headache (25%), difficulty in concentrating (15%), drowsiness (50%), warm glow (8%), relaxation (9%), fatigue (18%) and sleep (10%).

Beecher (3) summarizes his convictions as the result of several years study of the placebo problem by observing that the important factor in the "stimulus-suffering sequence" is a person's reaction to sensation. The action of a placebo or of a drug

which alters subjective responses to painful stimuli is through a modification of a reaction to an original sensation, rather than by direct effect on the original sensation itself. He cites as an example:

> Great wounds with greater significance and presumably great reaction are made painless by small doses of morphine, whereas fleeting experimental pains with no serious significance are not blocked by morphine. The difference here in the two situations would seem to be the difference in the significance of the two wounds. Morphine acts on the significant pain, not on the other. . . . It is difficult to explain in any other way how frontal lobotomy or barbiturate can relieve pain other than by altering the reaction to pain sensation. Comfort and pain relief can be separated by a barbiturate, by morphine and by prefrontal lobotomy. (My pain is the same but it doesn't hurt me now). The pain apparatus functions, but the disturbing element is blocked.

Out of Beecher's studies has also evolved an operational definition of the term "placebo" as applied to the experimental study of drug action. Gaddum (12) suggests that the function of the placebo used in this manner is twofold: as an aid in distinguishing pharmacological effects from the effects of suggestion, and as a controlling factor in the unbiased assessment of experimental results. To this Beecher (2) adds the stipulation that the use of placebo must be of a double blind nature, that is, undetected as a placebo by the observer as well as the subject.

In order to develop experimental procedures as free as possible from contaminating effects, the use of placebos in modern psychiatric research has recently become more extensive. The placebo control has been followed by the double blind method of investigation. Hoch (17) feels that this method is far superior to previous, purely clinical method of investigation in which the psychiatrist is totally unaware of the relative effects of those suggestive forces emanating from the type of compound administered, the internal milieu of the patient himself, and the very presence of the investigator in the setting. Although the superiority of the double blind method is widely recognized, it is generally accepted that even this method is not quite ideal. This fact prompts Hoch to make the suggestion that patients in an ideal experimental setting would not even know they were receiving any medication at all by giving it with the food if that were possible. The double blind method, as we know it today, is therefore not entirely free from the placebo effect, which is variable in different individuals. Masserman (23) concurs with this viewpoint and likens the double blind technique to what lawyers calls a "pretend rule"; that is, it refers mainly to an experimental conscience only, since frequently one can usually tell what drugs are being given by their direct or side effects.

As an example of the double blind technique, Masserman cites the work of Stewart, *et al.*, in which the effects of moderate doses of sugar and lactose were compared with corresponding doses of mephenesin. Purportedly, neither the doctors nor the patients knew which substance they were getting. The results of grading by this technique showed that small doses of the lactose had much greater effects, both salutary and adverse than mephenesin itself. As a further example of the complex results of the placebo phenomenon when observed in a double blind manner, Masserman refers to the work of W. B. Tucker who administered saline instead of streptomycin to patients ill with various infectious diseases. He then observed not only decreases in fever and diminution of white counts, but also various effects from saline comparable to those resulting from streptomycin, i.e., eosinophilia and impaired urea clearance.

Factors in placebo reactivity

While it would seem most logical that the subject of placebo reactivity is an area into which psychiatrists would have ventured earlier, it was really through the pioneering efforts of investigators outside the field of psychiatry that the first organized study of the placebo effect began. This is probably due to the fact that the organicist has rather concrete somatic end points against which he can contrast the results of his treatment variables, whereas, the psychiatrist, in his search for effective

techniques in the treatment of psychiatric patients is confronted with the experimental dilemma that the changes which he is trying to effect on the psychic processes may actually be nullified or even reversed by the psychic status of the patient. For instance, Wolfe (29) has pointed out how anger, hostility, or resentment greatly modified the action of a standard drug, such as atrophine, on the motor and secretory activities of the stomach or colon. Also, lactose and tapwater were demonstrated under certain emotional conditions to be physiologically active, and through suggestion ipecac abolished nausea. Organ behavior is therefore shown to be the resultant of many forces acting simultaneously, and the pharmacological actions of a drug may either reinforce or oppose these forces according to internal psychic, as well as somatic conditions.

It is a well-known, yet puzzling fact that reaction to placebo is by no means uniform among patients. The *Journal of the American Medical Association* (5) editorializing on the placebo, states: "What constitutes a good placebo cannot be specifically stated, because what may be good for one patient may not be so for another even though he has a similar condition." The uniformity of placebo reactivity is further complicated by changes resulting from patients' previous experiences with drugs and from the variance in the duration of time they are subjected to placebos. Lasagna (20) quotes Lindemann and Clark (22) who elaborate lucidly on this particular topic:

> Each drug undoubtedly has certain characteristics, but these are quite closely related to the conditions of the patients which are present when these specific effects are produced. The changes produced by a given drug will not only be elaborated on in the light of pre-existing psychic state, but totally new types of reaction may result from such an interrelationship.

Investigations attempting to answer the above questions have been performed by Hampson *et al.* (16) and by Lasagna *et al.* (19). The former finds that the greatest decrease in distress following placebos is experienced during the first two-week period. After that, a slight but statistically insignificant rise in distress occurs. At the end of eight weeks the placebo effect is again as great as after two weeks. The latter investigators find that with patients having surgical wounds the continued use of placebos for the relief of pain eventually results in a decreasing amount of relief experienced.

From the results of their study on placebo reactors, Tibbetts and Hawkings (28) make the following observations regarding the placebo response. Youth is positively related to placebo response, but there is little or no relationship between response and sex, intelligence, work record, presence of environmental problems, and severity of illness. Greater duration of illness prior to treatment and unremitting pattern of illness are negatively related to the response. Patients with unelaborated anxiety were most likely to respond positively to placebos. And somatic elaboration with a physiological basis does not greatly prejudice this response, but hysterical conversion affects it adversely—the fuller the conversion, the more resistant the patient.

Since the placebo reaction is generally accepted to be a manifestation of suggestion, the finding that it does not appear in hysterical conversion is at variance with the popular belief that there is a special relationship between hysteria and suggestibility. By way of explanation for this, Tibbetts and Hawkings explain that certain types of suggestion are more effective with patients who wish to improve, such as dysthymics, and not with those who are receiving powerful secondary gains and do not wish to give up meaningful symptoms.

Gantt and his co-workers (14) have investigated some of the conditional reflex factors influencing placebo reactivity. In their work with dogs they find that "persons" have different effects on animals, and that these effects can be conditioned. It is suggested that something comparable to this may occur in patients who are undergoing psychiatric treatment. Laboratory studies on "central excitatory states" indicate that these states are essential for conditioning to take place, therefore, the effectiveness of actual therapeutic proce-

dures, be they placebo or otherwise, may rely to some degree on the ability of doctors to arouse or excite their patients. The importance of the factor of conditioning is indicated by another notable experiment. For example, a dog was given apomorphine by injection and vomiting was produced. Eventually by conditioning, saline injections were made to bring about this same reaction, until finally the mere entrance into the room precipitated the reaction.

Personality and reactivity

The empirical knowledge regarding the placebo effect has assumed more definitive formulation as a system of personality information has developed as a result of the pioneering studies of Freud (10) on personality structure. The particular technique of personality study advocated by this brilliant theorizer has now become an important tool in the investigation of the psychopharmacology of drugs and the placebo. As a result of the psychoanalytical impetus there have been more and more personality studies on those patients responding to placebos, since this theoretical system has provided a conceptual framework for comparing the various personality elements which contribute to the general make-up of the placebo reactor and the non-reactor.

Lasagna and his co-workers (20), attempting to answer the question, "What makes a person a reactor?" agree that suggestibility is necessary, but argue that there are other factors which play an influential role, but which are not entirely discernible.

Pursuing this problem further, Lasagna, Von Felsinger, and Beecher (21) carried out personality studies on the subjects who deviated markedly from an expected response to series of drugs being used. From their study of a group of 69 patients who received two or more doses of placebo, 55% behaved inconsistently; 15% were consistent reactors and 31% were consistent non-reactors. They make the following conclusions regarding inconsistent reactors:

(1) They are immature persons dominated by impulsive and egocentric tendencies and inadequately controlled emotions. Control functions of the personality and socialized habits are not sufficient to balance impulsiveness. The person is frequently consciously aware of this problem, and external behavior can thus be deceptively controlled under ordinary non-stressful circumstances.

(2) They show strong, but diffuse striving for achievement and prestige beyond their capabilities. This striving is frequently unrealistically diverted, often into fantasy.

(3) Additional signs of immaturity and precarious control of behavior, such as indicated on the Rorschach by pure C responses, and depressive signs, such as moodiness or recurrent depressed periods, as well as Rorschach C′ responses, are more frequently encountered in this group than in the other groups.

(4) They constitute the most anxious and hostile members of the entire experimental population. All are above the median in anxiety-hostility scores.

The same investigators summarizing their efforts to screen out some of the more important psychological variables associated with the euphoria-dysphoria and stimulated-sedated reactions, feel that the differential personality characteristics of typical and atypical reactors suggest such a hypothesis as noted above. They find atypical responses most frequent in the least balanced persons and argue that it is not unreasonable to assume that such a personality structure, already inadequate to deal with everyday stresses, fraught with impulsivity and anxiety, and fearing loss of control, may be alarmed by the surge of stimulation and energy associated with a drug such as amphetamine. This then adds to the tension and further threatens precarious controls. On the other hand, a well-balanced personality structure conceivably integrates such stimulation into on-going activity without disturbance and with pleasant affect.

Even with the well adjusted, however, the effects of unknown and uncontrollable factors are felt in the placebo test situation. In a very interesting study, Gottschalk *et al.* (15) used a group of sophisticated observers, a team of psychologists and pharmacologists, to study the effect of the drug Pipradrol (Meratran). Each participant took varying doses of the drug and kept notes of his own subjective and behavorial responses, while another member of the group made observations on the subject's reactions. It was the purpose of the study

to see how accurately the subjects and observers could judge whether Pipradrol or a placebo had been taken. After two preliminary periods of familiarizing themselves with the specific subjective reactions they had to the drug, the subjects demonstrated a sound ability ($p < .001$) to discriminate between Pipradrol and a placebo in a double blind experiment. Forty-seven out of 60 trials were judged correctly. Results of attempts to discriminate between the effects of Pipradrol and a placebo through a combination of interview and observation of subjects were also significantly better than would be expected by chance ($p < .001$). This time 45 judgments out of 60 were correct. Errors in judgment were found to arise from factors in the personality make-up of the subject, the personality of the outside observer, and the personal interaction of subject and observer. Individual differences were noted in the dosage level at which subjects could detect the effects of the drug. Also, the psychological effect of the drug differed according to individuals and even differed in the same individual at different times.

Wolfe and Pinsky (30) observed the effects of placebos in patients with anxiety and tension and noted a 30% subjective improvement in their patients. However, when objective signs of anxiety were studied, such as tremulousness, sweating, and tachycardia, they found only a 17% improvement. Three out of the thirty-one patients had major reactions to the placebo; one promptly had overwhelming weakness, palpitation, and nausea, both after taking the placebo and after the tested ineffective drug. A second patient developed, while on the placebo (lactose), a diffuse rash—itchy, erythematous, and maculopapular, diagnosed as "dermatitis medicamentosa" by a dermatologist. A third patient, within ten minutes after taking the pills, developed epigastric pain followed by watery diarrhea, urticaria, and angioneurotic edema of the lips. This reaction was brought about on three separate occasions following the administration of the same placebo.

TABLE 1

TREATMENT STUDIES ON SCHIZOPHRENIC PATIENTS, PLACEBO USED

Investigator	Study	No. of Patients and Treatment	No. of Patients Improved
Funkenstein & Meade (11)	Study and Evaluation of Intravenous Ether as a Treatment for Psychiatric Patients (Primarily Affective Psychoses and Psychoses with Depressive Features.)	37 Intravenous Ether	14 (37.8%)
		17 Intravenous Saline	9 (52.9%)
		19 ECT	17 (89.4%)
Schrut (25)	Controlled Study on Clinical Use of Reserpine in Psychotic Patients with Special Comments On Sources of Error	151 Reserpine	35 (23.2%)
		44 Placebo	2 (4.5%)
Hollister, Traub, & Beckman (18)	Psychiatric Use of Reserpine and Chlorpromazine in Double Blind Studies	20 Reserpine	13 (65%)
		20 Placebo	3 (15%)
		24 Chlorpromazine	13 (54.1%)
		18 Placebo	3 (16.6%)
Freedman, Effron, & Bender (9)	Pharmacotherapy in Children with Psychiatric Illness	21 Benadryl	11 (52%)
		8 Ambodryl	3 (38%)
		14 Thorazine	8 (57%)
		8 Tolserol	3 (38%)
		14 Artane	6 (43%)
		10 Serpasil	4 (40%)
		13 Placebo	5 (38%)

Placebo reactivity in the psychotic

If one takes a sampling of chronically hospitalized psychotic patients and follows it over an extended period of time, some of these patients tend to show an appearance of improvement without receiving any medication at all. Galioni, Adams, and Tallman (13) demonstrate this by using a control group of 200 patients and a test group of the same number of patients who were exposed to a total push program. They found that 10 patients (5%) of their control group showed improvement due to an instinctual drive toward mental health in a very routinized environmental setting. The change factor producing an improvement change in the control group was augmented in the test group by the stimulation resulting from a total push program, so that 27 patients (18.5%) in this group were listed as improved.

With this awareness of the level of spontaneous movement which occurs, it became of interest to contrast these results against the reactivity obtained in some of the recently reported drug studies dealing with the treatment of psychotic patients which had been controlled with a placebo technique. In a sampling of four such studies, as indicated in Table 1, the placebo reactivity reported ranged from approximately 4% to 52%. It would thus appear if both approaches are contrasted that by inspection the lower level of placebo reactivity reported might well be that of spontaneous movement and have nothing to do with the placebo at all. The higher ranges of placebo reactivity would then tend to indicate that the added impact of the administration of a drug (although placebo) calls into play a reaction toward health. There is also the realization that this range is also effected by the variables in each study.

A survey of the literature failed to reveal any specific reports dealing with the psychodynamics of the reaction in the psychotic patient. The psychotic placebo-reactor may react quite differently. Apparently in this area of research the investigators were so preoccupied with the comparative studies of the drugs being used in treatment that the placebo effect observed in some of the patients found itself from the standpoint of investigation in the paradoxical situation of "the baby being thrown out with the bath water."

In all the reports concerning the psychotic placebo-reactor which have been reviewed none was found in which an absence of any placebo reactivity was reported. On the other hand, it might also be stated that there is no information available either on those patients who are made worse by placebo administration. This is a feature which is rarely reported. As far as we were able to learn there have been no personality studies of such patients reported.

DISCUSSION

In this report no attempt was made to present any data relating to placebo reactivity in those psychiatric disorders associated with the syndromes resulting from brain pathology, such as the acute and chronic brain syndromes. Brief mention will just be made that placebo reactivity has been reported in the mentally deficient, senile psychosis, and lobotomized patients. The degree of organicity which must be present before the placebo effect is lost as yet has had no clear formulation. What a comparison between the functional and nonfunctional disorders might indicate as to their placebo reactivity is something upon which we can only speculate at the present time. It would be our guess that the ranges reported might ultimately turn out to be the same.

In the treatment of patients with drugs, much remains to be learned about the patients' attitudes, feelings, and fears. Ayd (1) has reported in patients receiving tranquilizing drugs for various psychiatric disorders that it is not an infrequent occurrence to have such patients develop a state of near panic about the medication after taking only a single dose or two. Some patients taking small doses of chlorpromazine soon begin to experience feelings of depersonalization and unreality associated with restlessness and mounting anxiety. This occurs most in patients with marked obsessive features. Anxiety without its somatic complaints can occur in some patients in whom chlorpromazine causes mental lethargy and weakness. These patients usually cannot tolerate the lack of

energy to measure up to their self-imposed standards.

Another factor which may play a role in placebo reactivity and which has to be further elucidated is the harshness of the treatment procedure. For example, what is the relationship of placebo reactivity to drugs versus such somatic treatment procedures, such as electro-convulsive therapy, insulin coma therapy, and carbon dioxide?

References to other modalities of treatment other than drugs have been excluded, such as the relationship between psychotherapy and the placebo effect, as reported by Rosenthal and Frank (24) who state, "It is concluded that improvement under a special form of psychotherapy cannot be taken as evidence for: (*a*) correctness of the theory on which it is based; or (*b*) efficacy of the specific technique used, unless improvement can be shown to be greater than or qualitatively different from that produced by the patient's faith in the efficacy of the therapist and his technique—'the placebo effect'."

Other indications for placebo control research may be found in the conditional reflex field. Most investigators in this field are inclined to feel that the conditional response to a stimulus is not necessarily determined solely by the stimulating agent itself. The same signal may produce at different times (according to the inner state of inhibition) a positive, a negative, or no reaction. In considering this subject, it is obvious that the placebo in some way or another has acquired additional properties besides those that are inherent in itself. In other words, the placebo has become a conditional stimulus. It seems clear that when a placebo gives a specific reaction unwittingly, a technique has been adopted which produces a conditional reaction. For example, patients in our conditional response laboratory soon acquire accessory conditional reactions to the experimenter, the procedure, and the room in which the experiments are conducted. This, of course, would escape notice if we were not recording their responses. Furthermore, the behavior would be bewildering as the changes revealed in the salivary record could not be correlated with these and also extraneous chance stimuli.

The techniques of producing conditional reaction are quite involved. While they take place automatically it may not be apparent immediately how the anticipated, the opposite, or no reaction may be produced using what seems to be the identical stimulus and the same reinforcement. The first of these reactions represents a positive conditional reflex. The second is a negative reflex. The third is a neutral reaction. Failure to distinguish between these reactions has resulted frequently in unwarranted conclusions. It cannot be said, for example, that the patient is not responding to a placebo when no specific response is elicited. The matter of the inner state of the patient, the timing of the stimulus, the reinforcement, and the presence of extraneous stimuli can account for the lack of uniform responses.

With the psychiatric and psychologic techniques at our disposal, the evaluation of the inner state or state of awareness of the patient constitutes an ill-defined presentation. The inner state seems to be a mixture of conscious and unconscious processes out of which evolves a behavioral pattern. The technique of conditional reflexes provides an instrument which indicates changes within the inner state of the patient in terms of the inhibitory process without the conscious elaborations becoming manifest. By having an index of the state of internal inhibition we may be able to disentangle ourselves from the issue of whether we are dealing with an unconscious phenomenon or some reflection of the conscious awareness of the patient. What we need to know is the extent of the internal inhibition and where it is distributed within the nervous system.

Details of how this is being accomplished do not fall within the province of this paper. Briefly, however, through the use of the salivary conditional reflex in man, Sutherland (7, 8, 26, 27) has come to the conclusion that this method furnishes a relatively uncomplicated technique and is less influenced by extraneous stimuli such as the other techniques of studying conditional responses, namely, heart rate, respiration, psychogalvanic skin response, blood pressure, and changes in pupillary diameter. It might be added at this point that as the result of the

application of this technique, an experimental program has developed in which patients who have been subjected to a variety of different types of psychiatric treatment, including lobotomy, are being studied. This last group is now being studied from the standpoint of placebo reactivity.

Tentative impressions are all we can offer at this time with assurance. These are that placebo reactivity may be cyclical in character, with the forces bringing this about as yet largely unknown. In general, it may be stated that in the younger patient there is more of a tendency toward placebo reactivity. There seems to be no difference in the range of the placebo reactivity reported in psychotic and nonpsychotic patients. There are indications that the type of control exercised by the personality structure is related to placebo reactivity. Finally, it would seem, as a result of this investigation, that a fruitful area of exploration for future study would be that of the personality structure of placebo reactors per se and in terms of their conditional responses.

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The castiron determinism of primary law is, I think, still widely accepted but no longer unquestioningly. It now seems clear that we have not yet got hold of *any* primary law—that all those laws at one time supposed to be primary are in reality statistical. No doubt it will be said that that was only to be expected; we must be prepared for a very long search before we get down to ultimate foundations, and not be disappointed if new discoveries reveal unsuspected depths beneath. But I think it might be said that Nature has been caught using rather unfair dodges to prevent our discovering primary law—that kind of artfulness which frustrated our efforts to discover velocity relative to the aether. I believe that Nature is honest at heart, and that she only resorts to these apparent shifts of concealment when we are looking for something which is not there. It is difficult to see now any justification for the strongly rooted conviction in the ultimate re-establishment of a deterministic scheme of law except a supposed necessity of thought. Thought has grown accustomed to doing without a great many "necessities" in recent years.

—A. S. EDDINGTON in *The Nature of the Physical World*

ON THE CONCEPT OF EQUAL EXCHANGE[1]

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A system is said to be in equilibrium when no changes are perceived in it by gross observation. Yet there may be vigorous activity within such a system which is hidden from gross observation, because the inner processes "cancel" each other. Thus, a number of cities can maintain constant populations even though the birth, death, and migration rates are high. A system will be obviously in equilibrium if there is "equal exchange" between every pair of its subsystems, but not all systems in equilibrium are such systems of "strict exchange." It is a question of some theoretical importance whether a system is a system of strict exchange. Mathematical conditions for such systems are given here. In particular it is shown that a "perfectly mobile society," i.e., one where the probability of a person's position is independent of that of his father, is a strict exchange system.

THE social scientist is often faced with problems which involve the study of different types of systems of exchange. Such problems frequently take the following form: A system with a given number of states (cities, social classes, role-positions in groups) is isolated for study. A "social process" which interrelates the states of the system is then analyzed. The social scientist is then concerned with the effects of the social process on some characteristics of the states of the system.

For example, a demographer might be interested in the population sizes of a given set of cities. Let us assume that investigation has shown him that variations in the population size of each city can be accounted for by the yearly intercity mobility process within the set[2]. For his purposes then the social process which is interrelating the states of his system is the yearly pattern of population movements from city to city. In effect this type of problem can be conceived of as involving the study of a system of exchange—where individuals are "being exchanged" between the states of the system in given time intervals.

Viewed from this perspective, our hypothetical demographer's problem may be seen to have many features in common with a type of problem which might be of interest to a small group investigator: studying the exchange of action-units, in a given time period, as between individuals in different role-positions in a group; or a problem which a stratification theorist might be concerned with: studying the exchange of family units as between the occupational classes of a social structure in a given number of generations.

Now in each case, if the social scientist is in a position to make some assumptions about the stability of the exchange process—for example, that the pattern of intercity population movements does not change from time unit to time unit, or that the social mobility process is relatively constant from one generation to the next, etc., it may be pos-

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[2] To simplify our illustration, we assume that there are no major differences in the birth and death rates of the cities in the set, and that there are no major differences in net migration rates of the cities in the set with areas outside the set.

sible for him to raise some interesting questions concerning the exchange properties of the system.

1. If the particular system under study achieves an equilibrium condition will it be true that the units which are being exchanged will be exchanged in "equal amounts" as between every two states (or components) of the system? Consider the intercity mobility illustration, for example: Will a condition of equilibrium in the system imply that the number of individuals who move from any one city to any second city will be equal to the number who move from the second city to the first in a given time unit?

2. What are the characteristics of a process which imply that if the system achieves an equilibrium condition it will be the case that there will be equal exchange of the units as between every two states of the system, for each subsequent time interval? In terms of our intercity mobility case the question being raised is: What type of intercity mobility process will, in fact, lead to a situation of equal exchange of individuals as between every two cities in the system for each time period?

3. If a particular social process is one such that under conditions of equilibrium it entails a pattern of equal exchange of units, in a time interval, as between every two states, what inferences can be made about the nature of the process?

To deal with such questions it is necessary that we take such a concept as equal exchange, which has intuitive appeal in the analysis of social processes, and submit it to formal investigation. If it is possible to define such a concept in terms of a fairly general formal system, and then to investigate its properties, it would be reasonable to expect that its value as a tool of analysis, for the social scientist, would be considerably increased.

This paper represents an initial attempt to deal with this problem. Specifically in section A, the concept of equilibrium, as it applies to a specific class of processes, is developed. In sections B to E, the concept of equal exchange is defined, and its properties are investigated. In section F, the equal exchange model is employed in examining a specific sociological problem: a problem in intergenerational mobility.

It is hoped that the formal investigation of the concept of equal exchange may lead to the study of various types of social processes in terms of this perspective.

A. INTRODUCTORY CONCEPTS

By way of introducing some of the basic concepts that we shall be employing in the formulation of the model for equal exchange processes, we shall first consider a specific illustration of the type already alluded to: a case in the area of intercity population movements.

Let us assume that we are interested in studying the intercity population movements of three cities X, Y, Z. Each city sends, within a one-year time period, a certain fraction of its population to itself and to the other cities, as indicated in Figure 1. These fractions may also be represented by a matrix

$$P = \begin{array}{c} \\ \text{City X} \\ \text{City Y} \\ \text{City Z} \end{array} \begin{array}{c} \begin{array}{ccc} \text{City X} & \text{City Y} & \text{City Z} \end{array} \\ \begin{pmatrix} .4 & .2 & .4 \\ .4 & .2 & .4 \\ .1 & .3 & .6 \end{pmatrix} \end{array}$$

The initial populations of these cities may be represented by a vector $m = \{m_X, m_Y, m_Z\}$ where m_X is the population of city X, m_Y the population of city Y, and m_Z the population of city Z.

Let us suppose that the initial population

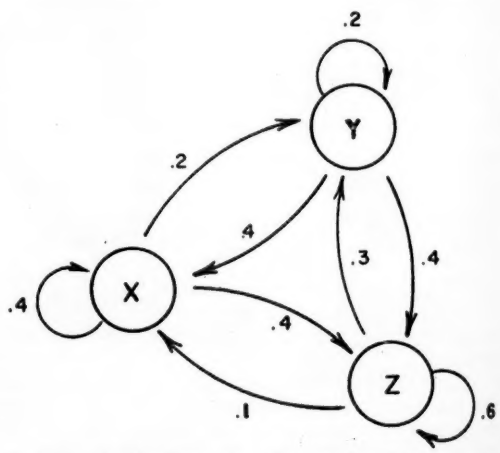

FIG. 1. Yearly Intercity Population Movements of a Three City System, X, Y, Z.

sizes are: city X = 2500, city Y = 5000, city Z = 2500. Then the population sizes after the first year can be obtained by multiplying the matrix P on the left by the vector $m = \{2500, 5000, 2500\}$. This multiplication is carried out as follows:

$$\begin{aligned}&(2500, 5000, 2500)\cdot P\\ &= (2500 \times .4 + 5000 \times .4 + 2500 \times .1,\\ &\quad 2500 \times .2 + 5000 \times .2 + 2500 \times .3,\\ &\quad 2500 \times .4 + 5000 \times .4 + 2500 \times .6)\\ &= (3250, 2250, 4500).\end{aligned}$$

The population sizes after the second year will then be $\{3250, 2250, 4500\}\cdot P = \{2650, 2450, 4900\}$. The same result could be obtained by multiplying the matrix P^2 on the left by the initial vector. For a discussion of basic matrix operations see Reference 6.

For a more general analysis we may define this type of situation in the following terms:

We assume that we have a set of r elements $\{a_1, a_2, \cdots, a_r\} = U$. The elements a_j are called *states* of the system. With each state at any given time we have a nonnegative number m_j called the *mass* of the state. We denote the set of masses by the vector

$$m = \{m_1, m_2, \cdots, m_r\}$$

and the total mass by

$$M = m_1 + m_2 + \cdots + m_r .$$

We imagine our system observed at a sequence of times and the mass vector changing in time. We assume that this change can be represented by a matrix P. More precisely, we assume that, if the mass vector at one time is $m^{(0)}$, then the mass vector at the next time will be given by $m^{(1)} = m^{(0)}\cdot P$. The matrix P has the property that the entries are nonnegative and have sum one. The element p_{ij} represents the fraction of the mass in state a_i which is sent in a unit time to state a_j. The entry $p_{ij}^{(n)}$ of the matrix $P^{(n)}$ represents the fraction sent over an n unit time interval.

Definition. A mass distribution m is said to be an equilibrium distribution if $mP = m$.

It is clear that, if a system ever has an equilibrium distribution, it will have this mass distribution for all later times. In this case we shall say that the system is in equilibrium. If P is as in our three-city illustration, the vector $m = \{2500, 2500, 5000\}$ is an equilibrium distribution. Notice that this distribution is near that which was obtained after only two time periods for the initial distribution $m = \{2500, 5000, 2500\}$. The following theorem from the theory of Markov chains (1) describes the behavior of a large class of processes of the type which we are considering.

Theorem 1.0. Assume that the matrix P has the property that for some N, P^N has no zero elements. Then P^n approaches a matrix T where T is a matrix having each row the same vector $t = \{t_1, t_2, \cdots, t_r\}$. The vector t is the unique vector having the sum of its components equal to one and such that $tP = t$.

It follows from Theorem 1.0 that if P satisfied the condition of this theorem, then no matter what the initial mass distribution is, as time goes on, the distribution will approach a mass distribution proportional to t. Note that the theorem states that the fraction of the mass in a state a_i which will be sent to state a_j after a large number of unit time intervals is approximately t_j independent of the state a_i considered.

B. DEFINITION AND PROPERTIES OF EQUAL EXCHANGE

We assume now that P satisfies the condition of Theorem 1.0 and the system is started in equilibrium, i.e., initial mass vector m has the property that $mP = m$.

We interpret $q_{ij} = m_i p_{ij}$ as the mass which a_i sends, in a unit time interval, to a_j. A natural question to ask is the following: In equilibrium, is the mass which a_i sends to a_j equal to the mass which a_j sends to a_i? That is, does $q_{ij} = q_{ji}$? The answer to this question is, in general, no. However, there are important applications where one would expect that this would be true or at least approximately true. To examine these "exchange" properties of a system, we define the *exchange matrix* to be the matrix Q with entries q_{ij} and the exchange difference matrix Δ as the matrix with entries $\delta_{ij} = q_{ij} - q_{ji}$.

In the example given in the introduction, with equilibrium mass vector $m = \{2500,$

2500, 5000}, we obtain

$$Q = \begin{pmatrix} 1000 & 500 & 1000 \\ 1000 & 500 & 1000 \\ 500 & 1500 & 3000 \end{pmatrix}$$

and

$$\Delta = \begin{pmatrix} 0 & -500 & 500 \\ 500 & 0 & -500 \\ -500 & 500 & 0 \end{pmatrix}$$

We note in Q that the sum of the ith row and the ith column is equal, and their common value gives the equilibrium mass associated with state a_i. In Δ, each row and each column have sum 0. Both of these facts are true for general Q and Δ matrices, and are consequences of the fact that in equilibrium the amount of mass which a state sends out must be the same as it receives.

We now extend the concepts of equal exchange to arbitrary subsets R and S of the set of all states U.

Definition. Two subsets, R and S of U are said to be in equal exchange if the mass which R sends to S is equal to that which it receives from S. That is $\sum_{i \in R, j \in S} \delta_{ij} = 0$.

From the fact that the sum of each row of Δ is zero we have the following two theorems:

Theorem 1.1. Any subset R of U is in equal exchange with its complement $\bar{R}$.

Theorem 1.2. Let R, S, and T be mutually exclusive and exhaustive subsets of states. Then, if any pair of R, S, and T are in equal exchange, all pairs are in equal exchange.

Definition. A subset R of U is said to be a *strict exchange subset* if every pair of elements of R are in equal exchange.

Let R be a strict exchange subset of U and A be a subset of R. Then, by the definition of strict exchange, subsets $\bar{A} \cap R$, and A are in equal exchange. Applying Theorem 1.2 to the sets A, $\bar{A} \cap R$, and $\bar{R}$ we have the following theorem:

Theorem 1.3. Let R be a strict exchange subset of U and A a subset of R. Then A is in equal exchange with $\bar{R}$.

We shall say that a *system* determined by a matrix P is in strict exchange if every pair of states is in equal exchange, i.e., if the set of all states U is a strict exchange set. A system is said to be in partial strict exchange if at least one pair of states is in equal exchange, i.e., if there is at least one subset of U which is a strict exchange subset. If a system is in strict exchange, the exchange matrix Q will necessarily be a symmetric matrix and the difference matrix will be the zero matrix.

We note that by Theorem 1.1, any two-state system is necessarily in strict exchange. By Theorem 1.2 a three-state system is in strict exchange if two of the states are in equal exchange.

Example 1. Let P be a symmetric matrix, i.e., $p_{ij} = p_{ji}$. Then the limiting mass vector assigns to each state a_i the same mass $m_i = c$. Thus $m_i p_{ij} = cp_{ij} = cp_{ji} = m_j p_{ji}$. Therefore, the system is in strict exchange.

Example 2. Let

$$P = \begin{array}{c} \\ a_1 \\ a_2 \\ a_3 \\ a_4 \end{array} \begin{array}{c} \begin{array}{cccc} a_1 & a_2 & a_3 & a_4 \end{array} \\ \begin{pmatrix} 0 & \frac{1}{6} & \frac{1}{3} & \frac{1}{2} \\ \frac{1}{4} & 0 & \frac{1}{2} & \frac{1}{4} \\ \frac{3}{5} & \frac{1}{5} & 0 & \frac{1}{5} \\ \frac{2}{5} & \frac{2}{5} & \frac{1}{5} & 0 \end{pmatrix} \end{array}.$$

Here the equilibrium distribution for a total mass of 30 is $m = \{6, 4, 5, 5\}$. Thus

$$Q = \begin{pmatrix} 0 & 1 & 2 & 3 \\ 1 & 0 & 2 & 1 \\ 3 & 1 & 0 & 1 \\ 2 & 2 & 1 & 0 \end{pmatrix}$$

and

$$\Delta = \begin{pmatrix} 0 & 0 & -1 & 1 \\ 0 & 0 & 1 & -1 \\ 1 & -1 & 0 & 0 \\ -1 & 1 & 0 & 0 \end{pmatrix}.$$

In this example, the subsets $\{a_1, a_2\}$ and $\{a_3, a_4\}$ are both in strict exchange, but the system as a whole is not in strict exchange, i.e., the system is in partial strict exchange.

An example of the assertion of Theorem 1.3 would be the fact that state a_1 is in equal exchange with the set $\{a_3, a_4\}$.

C. AN ALTERNATIVE INTERPRETATION FOR STRICT EXCHANGE

We have dealt so far with the problems which involve predictions to the future. That is, $p_{ij}^{(n)}$ represents the fraction of the mass in state a_i which will be in state a_j after n time units. We consider now the problem of relating the present to the past.

We assume that the system is in equilibrium and define π_{ji} to be the fraction of the mass at state a_j which came from state a_i in

a single time unit. Then

$$\pi_{ji} = \frac{m_i p_{ij}}{m_j}.$$

If we had not assumed that the system was in equilibrium this fraction would have depended on the particular time being considered.

Let Π be the matrix with entries π_{ji}. Then the entry $\pi_{ji}^{(n)}$ of the matrix Π^n represents the fraction of the mass of state a_j which came from a_i in n previous time units. If the matrix P satisfies the conditions of Theorem 1.0, then so will Π. Furthermore, if $mP = m$ then so does $m\Pi = m$. Thus by Theorem 1.0 the fraction of the mass in state a_j which came n generations ago from a_i is approximately $\frac{m_i}{m_1 + \cdots + m_r}$ if n is large. Note that this fraction is independent of the state a_j considered.

The matrices P and Π not only have the same fixed vector m, but they also have the same eigenvalues. This can be seen as follows: Let M be the diagonal matrix

$$M = \begin{pmatrix} m_r & & & & \\ & m_{r-1} & & & \\ & & \ddots & & \\ & & & \ddots & \\ & & & & m_1 \end{pmatrix}$$

and let P^T be the transpose of P. Then

$$\Pi = MP^TM^{-1}.$$

Thus P^T and Π, and hence also P, have the same eigenvalues.

Assume now that we have an equal exchange system. Then $\frac{m_i p_{ij}}{m_j} = p_{ji}$ which in turn implies that $\pi_{ji} = p_{ji}$. Thus we have an alternative description for an equal exchange process. An equal exchange process is a system in equilibrium such that the fraction of the mass of a_j which *came* from a_i in a unit time is the same as the fraction of the mass of a_j which is *sent* to a_i in a unit time.

A system with the property that $\pi_{ji} = p_{ji}$ for all i and j is called, in the theory of Markov chains, a reversible process (3). Thus reversibility and strict exchange are equivalent concepts.[3] Strict exchange for a subset A is equivalent to asking that the system be reversible with respect to the elements of A.

D. A CONDITION FOR STRICT EXCHANGE

In this section we shall prove a theorem which enables us to test strict exchange by the matrix P.

Theorem 1.4. Let P have all nondiagonal entries positive. A necessary and sufficient condition for such a P to define a strict exchange system is that for some fixed r and every pair k, j

$$p_{rj}\, p_{jk}\, p_{kr} = p_{rk}\, p_{kj}\, p_{jr}. \tag{1}$$

We prove first that if (1) is true for some fixed r, then this "cyclic" property holds for every triple i, j, k. By (1), for the sets (r, j, i), (r, j, k), (r, k, i) we have

$$p_{rj}\, p_{ji}\, p_{ir} = p_{ri}\, p_{ij}\, p_{jr}$$

$$p_{rj}\, p_{jk}\, p_{kr} = p_{rk}\, p_{kj}\, p_{jr}$$

$$p_{rk}\, p_{ki}\, p_{ir} = p_{ri}\, p_{ik}\, p_{kr}.$$

Thus

$$\frac{p_{ij}}{p_{ji}} = \frac{p_{rj}\, p_{ir}}{p_{ri}\, p_{jr}}$$

$$\frac{p_{jk}}{p_{kj}} = \frac{p_{rk}\, p_{jr}}{p_{rj}\, p_{kr}}$$

$$\frac{p_{ki}}{p_{ik}} = \frac{p_{ri}\, p_{kr}}{p_{rk}\, p_{ir}}.$$

Multiplying these ratios together we have

$$\frac{p_{ij}\, p_{jk}\, p_{ki}}{p_{ik}\, p_{kj}\, p_{ji}} = 1.$$

Thus (1) is equivalent to stating, for every i, j, k

$$p_{ij}\, p_{jk}\, p_{ki} = p_{ik}\, p_{kj}\, p_{ji}. \tag{1'}$$

We now prove that if (1) is satisfied, then, for every n

$$\frac{p_{ij}}{p_{ji}} = \frac{p_{ij}^{(n)}}{p_{ji}^{(n)}}. \tag{2}$$

[3] It is interesting to observe that from consideration of social processes we are led to a concept which is equivalent to one which arises more naturally from the consideration of examples in physical sciences. See Reference 3 for one such example.

The proof is by induction. The theorem is trivially true for $n = 1$. Assume that it is true for $n - 1$. Then

$$p_{ij}^{(n)} = \sum_{k \neq i} p_{ik}^{(n-1)} p_{kj} + p_{ii}^{(n-1)} p_{ij}.$$

Using the induction hypothesis we have

$$p_{ij}^{(n)} = \sum_{k \neq i} \frac{p_{ki}^{(n-1)} p_{ik}}{p_{ki}} p_{kj} + p_{ii}^{(n-1)} p_{ij}.$$

Then by (1′)

$$\begin{aligned} p_{ij}^{(n)} &= \sum_{k \neq i} p_{ki}^{(n-1)} \frac{p_{ij} p_{jk}}{p_{ji}} + p_{ii}^{(n-1)} p_{ij} \\ &= \frac{p_{ij}}{p_{ji}} \cdot \sum_{k \neq i} p_{ki}^{(n-1)} p_{jk} + p_{ii}^{(n-1)} p_{ij} \\ &= \frac{p_{ij}}{p_{ji}} \cdot \sum_{k} p_{ki}^{(n-1)} p_{jk} \\ &= \frac{p_{ij}}{p_{ji}} p_{ji}^{(n)}. \end{aligned}$$

If we now let n tend to infinity in (2) we obtain by Theorem 1.0

$$\frac{p_{ij}}{p_{ji}} = \lim_{n \to \infty} \frac{p_{ij}^{(n)}}{p_{ji}^{(n)}} = \frac{m_j}{m_i}$$

or

$$m_i p_{ij} = m_j p_{ji}.$$

Thus we have proved that if (1) is satisfied we will have strict exchange. Assume now that we have strict exchange. Then

$$\frac{m_i}{m_j} = \frac{p_{ji}}{p_{ij}} \tag{3}$$

$$\frac{m_i}{m_k} = \frac{p_{ki}}{p_{ik}}. \tag{4}$$

Dividing (4) by (3) we have

$$\frac{m_j}{m_k} = \frac{p_{ki} p_{ij}}{p_{ik} p_{ji}}. \tag{5}$$

But also, by assumption, we have

$$\frac{m_j}{m_k} = \frac{p_{kj}}{p_{jk}} \tag{6}$$

so that, by (5) and (6) we have

$$\frac{p_{kj}}{p_{jk}} = \frac{p_{ki} p_{ij}}{p_{ik} p_{ji}}$$

or

$$p_{ik} p_{kj} p_{ji} = p_{ij} p_{jk} p_{ki}.$$

Hence (1′) and consequently (1) is satisfied.

We observe that strict exchange is equivalent to requiring that the ratio $m_i p_{ij}^{(n)} / m_j p_{ji}^{(n)}$ be constant for all n. That is, the ratio of the amount that a_i sends to a_j and the amount that a_j sends to a_i should remain constant. We note also that in strict exchange systems the equilibrium masses are easily obtained from the transition matrix. In fact for any j they are proportional to the ratios

$$\frac{p_{j1}}{p_{1j}}, \frac{p_{j2}}{p_{2j}}, \cdots, \frac{p_{jr}}{p_{rj}}.$$

E. EQUAL EXCHANGE RELATIVE TO n TIME UNITS

So far we have considered the concept of equal exchange only for a unit time interval. It is easy to extend this to arbitrary time intervals.

Definition. The states a_i and a_j are in equal exchange relative to n time units if the system is in equilibrium and

$$m_i p_{ij}^{(n)} = m_j p_{ji}^{(n)}.$$

By a system in strict exchange relative to n time units we mean a system such that every pair of elements has equal exchange relative to this time interval. The following theorem follows from the proof of Theorem 1.4.

Theorem 1.5. If a system is in strict exchange for unit time intervals it is in strict exchange for any time interval.

It is not true that a system with partial strict exchange for unit time intervals will necessarily preserve the same exchange equalities for all time intervals. To illustrate this we shall show that in Example 2 of section B, the states a_1 and a_2 are in equal exchange for all time intervals but the states a_3 and a_4 are in equal exchange for unit time intervals but not for two unit time intervals.

We recall that for this example the matrix P was

$$P = \begin{array}{c} \\ a_1 \\ a_2 \\ a_3 \\ a_4 \end{array} \begin{array}{c} \begin{array}{cccc} a_1 & a_2 & a_3 & a_4 \end{array} \\ \begin{pmatrix} 0 & \frac{1}{6} & \frac{1}{3} & \frac{1}{2} \\ \frac{1}{4} & 0 & \frac{1}{2} & \frac{1}{4} \\ \frac{3}{5} & \frac{1}{5} & 0 & \frac{1}{5} \\ \frac{2}{5} & \frac{2}{5} & \frac{1}{5} & 0 \end{pmatrix} \end{array}$$

If we now find the corresponding matrix Π we have

$$\Pi = \begin{array}{c} \\ a_1 \\ a_2 \\ a_3 \\ a_4 \end{array} \begin{array}{c} \begin{array}{cccc} a_1 & a_2 & a_3 & a_4 \end{array} \\ \begin{pmatrix} 0 & \frac{1}{6} & \frac{1}{2} & \frac{1}{3} \\ \frac{1}{4} & 0 & \frac{1}{4} & \frac{1}{2} \\ \frac{2}{5} & \frac{2}{5} & 0 & \frac{1}{5} \\ \frac{3}{8} & \frac{1}{8} & \frac{1}{8} & 0 \end{pmatrix} \end{array}$$

We observe that P and Π are the same if in Π we interchange states three and four. Thus $p_{12}^{(n)} = \pi_{12}^{(n)}$ for all n. Moreover, since $\pi_{12}^{(n)} = \frac{m_2}{m_1} p_{21}^{(n)}$ we have $m_1 p_{12}^{(n)} = m_2 p_{21}^{(n)}$ for all n. Hence the states a_1 and a_2 are in equal exchange for all n. While it is true that $p_{34} = \pi_{43}$ it is not true, for example, that $p_{34}^{(2)} = \pi_{43}^{(2)}$ so that while a_3 and a_4 are in equal exchange for unit time intervals they are not for two unit time intervals.

F. APPLICATION TO A PROBLEM IN SOCIAL MOBILITY

It is to be remembered from our three-city example that not all processes which can be described in equilibrium terms need be cases of strict exchange.

We shall now consider an actual case of a rather significant type of social process which appears to exhibit strict exchange properties.

In an article entitled *The Formal Theory of Social Mobility*, Paris (5) has shown how the process of intergenerational social mobility can be considered from the standpoint of equilibrium analysis. Briefly the problem involves the following: (*a*) a set of occupations or occupational groupings (professional and high administrative, managerial and executive, etc.) are taken to represent the states of the system to be analyzed; (*b*) the intergenerational mobility process itself is described as a matrix P. Thus the p_{ij} entries in the matrix P represent the fraction of individuals whose fathers were in occupation i who have "moved" to occupation j.

Using data collected by Glass and Hall (2) on the occupational status of male residents in England and Wales and that of their fathers, Prais proceeds to estimate the actual p_{ij} values for a "social mobility matrix." Employing Theorem 1.0 he then proceeds to determine what, in effect, would be the equilibrium distribution for the given occupational structure.

In Table 1 is presented his matrix of transitional probabilities, the actual distribution in 1949, and the derived equilibrium distribution.[4]

TABLE 1

	Upper	Middle	Lower	Actual Distribution	Equilibrium Distr.
Upper	.45	.48	.07	.075	.062
Middle	.05	.70	.25	.634	.623
Lower	.01	.50	.49	.291	.314

To determine the special equilibrium properties of the type of system which Prais has described we first find the exchange matrix Q.

$$Q = \begin{array}{c} \\ \text{Upper} \\ \text{Middle} \\ \text{Lower} \end{array} \begin{array}{c} \begin{array}{ccc} \text{Upper} & \text{Middle} & \text{Lower} \end{array} \\ \begin{pmatrix} .028 & .030 & .004 \\ .031 & .436 & .156 \\ .003 & .157 & .154 \end{pmatrix} \end{array}$$

and then the exchange difference matrix Δ,

$$\Delta = \begin{array}{c} \\ \text{Upper} \\ \text{Middle} \\ \text{Lower} \end{array} \begin{array}{c} \begin{array}{ccc} \text{Upper} & \text{Middle} & \text{Lower} \end{array} \\ \begin{pmatrix} 0 & -.001 & .001 \\ .001 & 0 & -.001 \\ -.001 & .001 & 0 \end{pmatrix} \end{array}$$

The exchange difference matrix Δ reveals that the process described by Prais, under conditions of equilibrium, closely approximates the pattern of a strict exchange system.[5]

Considered from the standpoint of recruitment patterns of each class, our results indicate that if this occupational system attains equilibrium under these conditions, it will be the case, that the fraction of individuals in any occupational class i, who have been recruited, in one time period, from any other occupational class j, π_{ij}, will tend to equal

[4] To simplify his analysis, Prais has grouped the original occupational classes of Glass and Hall in accordance with the following scheme: *Upper:* Professional, high administrative, managerial, executive; *Middle:* Higher grade supervisory and nonmanual, lower grade supervisory and nonmanual, skilled manual and nonmanual; *Lower:* Semiskilled manual, unskilled manual.

[5] An exchange analysis on the more detailed data, involving the original seven-class system reported on by Glass and Hall (2) and analyzed in terms of equilibrium properties by Prais (4), yields essentially similar results.

the fraction of individuals in occupation i who in turn will have moved to occupational class j, p_{ij}. This expresses the fact that a strict exchange process is a reversible process (section C).

Furthermore by Theorem 1.5, it follows that this pattern of *recruitment-equalities* will remain a characteristic of this class system for all subsequent time periods.

Let us now assume that this system has been in equilibrium for a number of time periods where each time period is taken to represent a generation. Then stated in probability terms, the reversibility characteristic of this process which holds for all time periods would mean, for example, that the probability that the *grandson* of a given member of class j will be in class i, is approximately equal to the probability that the *grandfaiher* of this given member of class j was in class i.

Considered from the standpoint of cyclic conditions, it would be true by Theorem 1.4 that in this type of mobility system the probability of moving from a given class i, through the other two classes of the system and returning to i, in three generations is independent of the direction traveled. Thus, for example, the probability of moving from upper to middle class, middle to lower class, and back to the upper class in three generations should tend to equal the probability of moving from upper to lower class, lower to the middle class, and back to the upper class in three generations. The matrix of transitional probabilities reveals that this condition is in fact approximated for this case.

As a final consideration, it is of some interest to examine briefly the exchange properties of what Prais has defined as the *perfectly mobile society:* "a society in which the chance of being in any social class does not depend on the social class of one's father" (5).

In terms of this conception of the perfectly mobile system, the probability of moving from any occupational class i to any occupational class j, in a given time unit, is independent of the class i considered, or $p_{ij} = p_j$. In this case the equilibrium mass for class j is $m_j = Mp_j$ where M is the total number of individuals considered in the occupational system. Thus $q_{ij} = m_i p_{ij} = Mp_i p_j$, and $q_{ji} = m_j p_{ji} = Mp_j p_i$. Therefore, $\delta_{ij} = 0$ for every a_i and a_j.

It is thus the case that a mobility process, which is defined in these terms as a system of perfect mobility, is in fact a strict exchange system. Consequently it will exhibit the properties of such a system which we have formulated in this paper.

Finally, it should be noted that the model treated in this paper and in Prais is clearly only a first approximation. A more realistic model would take into account differential reproduction rates, migration rates (where appropriate), etc., in the analysis of exchange processes. Prais notes the problem of differential reproduction rates and offers a possible solution (5).

We believe that a solution of this problem involves the formulation of a more general model of exchange process which combines branching type processes with Markov chain theory. The authors hope to be able to present such a model in the near future.

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ON THE MEASUREMENT AND DESCRIPTION OF GROUPS[1]

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The behavioral scientist, to the extent that he considers himself a scientist, must aspire to predict behavior and to do this he must identify observables, build theoretical constructs and decide how on the basis of observations and deductions future observables can be predicted. Each level of observation presents its own problems. Here data of human groups are examined in the light of the predictive task of the behavioral scientist. Three classes of measures are distinguished, the raw data of the response aggregate, the behavior indexes, and the so-called "endogenes," comprising the basic character of the group. These are listed in the order of presumably increasing stability but unfortunately also in the order of decreasing ease of ascertainability. It seems the more important is a measure the harder it is to get at. A strategy to deal with this situation is proposed.

RECENT survey articles and bibliographies (23, 28, 30) testify impressively to the increasing volume and scope of sociological and social psychological research on small groups. A great many methodological importations from the older psychological fields have been adapted to the purpose of measuring group behavior and essentially new techniques have been devised. At present, however, there is little evidence of any comparable effort in the direction of analysis and organization of group measurement processes themselves. The need for such an analysis increases, with a growing suspicion that studies conducted under different banners and on different sorts of groups may actually pertain to the same general phenomena.

The present report attempts to point up crucial issues in group measurement and to suggest a broad framework for classifying measures and properties of groups. Less attention will be paid to those measurement requirements (such as reliability and validity) which are common to all the sciences than to the peculiar demands of group measurement. Similarly, content aspects of group measurement that seem well explored are slighted in favor of possible areas of measurement that now appear underdeveloped. Wherever possible, techniques are illustrated by citations from the literature, but there are certain operations that seem to follow from the present analysis for which no examples are available. In such cases, the report becomes largely programmatic in nature.

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PREDICTION AND GROUP MEASUREMENT

In the present analysis it will be assumed that the primary objective of measurement is the prediction of group behavior from observed antecedent behavior and conditions. Descriptive or connotative richness of measures, for example, is considered important only to the extent that it brings into play a fund of personal experience that may assist the prediction process. Control or modification of group behavior is merely the application of certain specialized predictions. This somewhat austere ground rule leads rather directly to what appears to be a workable and suggestive framework for organizing group measurements.

Suppose that observation of a particular group under specified conditions, S_o, yields a set of behaviors (b_{ijk}) where the i subscript indicates chronological time or stimulus occasion; the j subscript indicates the generic form of the response; and the k subscript identifies the "agent" or group member(s) concerned. Such a collection of observations is in general of little value until we associate with it a measure or index which we shall denote by $X(b_{ijk})$. As commonly used, measures of this sort would be simple statistical properties of the behavioral manifold or set.

From the foregoing observations and numerical operations, we wish to predict to some index of interest in a future situation that may be encountered by the group, say S_p. Frequently this index will represent some criterion of economic significance such as the productivity of the group or some internal characteristic such as group "integration." We denote this predicted measure by $Y(b'_{ijk})$, noting however that it may well be the same measure as X.

Given this statement of the problem, minimum conditions for predictability seem to require a further set of quantities, $E_1, E_2, \cdots$, attaching to groups such that:

(a) $$X(b_{ijk}) = F_a(S_o; E_1, E_2 \cdots)$$

(b) $$Y(b'_{ijk}) = F_b(S_p; E_1, E_2 \cdots)$$

That is, the behavioral index obtained in the observation situation is a function, F_a, of situational variables and a set of enduring group characteristics denoted by the E's. The index obtained in the prediction situation is a (possibly different) function, F_b, of the new situational variables and the same group characteristics (cf. 12). The desired prediction to Y is possible if we can infer the values $E_1, E_2 \cdots$ from measured X values and information on F_a and S_o; then enter $E_1, E_2 \cdots$ into a new function, F_t, with new situational variables S_p. Fortunately, in most predictions of practical importance we are justified in making assumptions which effectively short-circuit this chain of inference and derivation. These simplifications are considered in the Discussion.

The above formulation suggests four fairly distinct classes of measurement of relevance to group processes:

1. **Response aggregates,** (b_{ijk}), are the raw data for social psychological research. As measures, their primary requirement is faithful reflection of a sequence of behaviors by group members. That is, the indices i, j, k should respectively order behavioral events (either internally or on some external scale such as chronological time); establish an equivalence relationship within a class of behaviors; and identify participating or affected group members. The term "aggregate" implies that we may choose to treat a block of molecular responses as a single unit.

2. **Behavior indexes**, (X, Y), require a somewhat more special definition. They are primarily statistics derived from fragmentary behavior sequences, and as such they should be estimates of more definite and stable quantities. We define a *behavioral index*, therefore, as any function of the response aggregate measures for which the standard error of estimate decreases uniformly over a specified range of sampling. Behavioral indexes are not, in other words, assumed to be constants but are estimated values that cluster about a point of convergence if they are successively sampled under comparable conditions.

3. **Endogenes** (E_1, $E_2 \cdots$) are those inferred properties of groups which, in a given situation, account for certain distinctive behavior patterns. They are analogous to personality constructs such as aptitudes and source traits. The term "endogene" is derived from "endogenous variable" as used in econometric theory and is not intended to imply more than is implied by that phrase. Unlike the *behavior index*, which has a restrictive range of invariance, *endogenes* are assumed to be strictly invariant over any conditions of sampling—unless indeed the group changes in character.

4. **Situational parameters** (S_o, S_p) are certainly the most neglected, both in research and in theory, of all the terms considered here. Historically, this seems to be true because such a large proportion of group research has been concerned with

groups that are supposed to carry their own built-in atmosphere around with them, such as conference groups, discussion groups, therapy groups, and the like. As a consequence, most of our knowledge of situational factors is vague and circular. For example, we may know that certain roughly defined situations tend to bring out certain behaviors such as aggression or leadership but we can't, in general, identify the critical stimulus properties of those situations. A full discussion of this problem is beyond the scope of the present report.

In the next three sections each of the terms relating directly to group behavior is examined with respect to present and potential applications.

SOURCES AND TYPES OF RESPONSE AGGREGATE MEASURES

A few preliminary remarks may serve to clarify the intended meaning of the term "response aggregate." Briefly, it denotes any response or behavior on the part of a group member or any combination of such responses which affords a feasible unit for direct measurement. To extend the meaning further, we do not distinguish between the behavior itself and the direct metric representation of the behavior. For example, if the only indication of aggressive behaviors in a group is a certain response on a questionnaire, that response is treated, for all practical purposes, as a response aggregate.

Initial consideration will be given to the general conditions under which response aggregate measures are obtained. Next, several of the more important measurement techniques are examined in turn, and finally a basis for substantive classification of response aggregate measures is proposed.

Relation between measuring instrument and group

A fundamental problem of measurement throughout the sciences is the extent to which the measurement process interferes with or influences the measured process. The terms to be used here to indicate the extreme degrees of interference are *natural* and *intrusive* measurement, but it should be emphasized that this distinction may not be crucial in itself (1). The reaction of a group to even the most patently artificial testing situation is to some extent indicative of natural group processes. Conversely, practically any measurement process requires the interaction of some external system with the group and it may be a trivial question as to whether this system is controlled by a psychologist or not.

With these qualifications, however, the distinction may be a useful one. There are certain advantages and disadvantages which attach broadly to the opposed forms of measurement. Generally speaking, natural measurement (which might include such techniques as covert observation, content analysis of communications, and *ex post facto* examination of records or minutes) permits greater range and representativeness in the behavior observed. It avoids the delicate problems of motivational control which attend most intrusive measurements, and it may be accomplished without interference in group activity of possible social significance. Finally, inferences from natural measurement may be more easily related to the continuing pattern of the group's environment.

The advantages of intrusive measurement (such as simulated laboratory tasks and questionnaires) lie primarily in the sampling control it affords. Thus it is possible to select certain behavior categories in advance and to insure equal opportunities and occasions for all sampled groups to exhibit these behaviors. For this reason, when the relevant behaviors for a given study are identified it is almost certainly more economical to measure them intrusively.

Nature of the measuring instrument

There are three general classes of instruments for obtaining raw data concerning group behavior: self-report by group members; observer report; and physical or situational records. Each of these will be examined briefly with examples.

Self-report. In this technique group members are asked to describe themselves, other group members, attitudes toward the

group, or general conditions within the group. Well-known examples of self-report techniques are the sociometric and reputational rating devices (15, 22, 24, 26). The chief advantage of this technique, theoretically, lies in the fact that group members will have experienced a much greater slice of the group's history than any external measure can encompass, and presumably will reflect this experience in their reports. The chief disadvantage in practice is that they do not do so. Group self-report is subject to the same defects and biases that plague individual personality inventories; and informal observation indicates that these may even be exaggerated and compounded in group measurement. It may be possible to refine self-report measures to a point where they are subject to unequivocal interpretation but at that stage they will have lost much of their original richness. A secondary disdavantage incurred in the use of self-report is the degree of sensitization to social processes which is built up when this technique is used successively. With all these faults, there is simply no other way of obtaining certain types of information and the self-report techniques can be expected to occupy a dominating position in social psychology for some time to come.

Observer report. This technique, like the foregoing, uses a human measuring instrument but, in this case, one who is not party to the group process being measured. Observers may be psychologically trained or briefed; "content" trained in the group process being investigated; or, they may be representative members of the group's social milieu. Examples of psychologically trained observers are the use of graduate students as observers in the Harvard (3, 4) and Rochester (10, 11) laboratories. Content or process trained observers are employed in obtaining superiors' ratings of group performance in military studies (e.g., 21). Environmental representatives have not been as widely used but the technique seems just as feasible as individual reputational tests.

It is probable that external observers can report on group processes with greater detachment and objectivity than can group members themselves. Also, it is possible, over time, to build up standards of reference in an external observer while the group member is always more or less confined to absolute judgments. On the other hand, the training of observers is a slow and costly procedure and there are strong temptations to accept less than optimal levels of observer proficiency and dedication. It seems fair to say that this technique, like self-report, is one of necessity rather than choice.

Physical measurement. Physical measurement techniques include all cases where a group process produces a direct impress on the environment without human mediation or interpretation. Most "objective" performance measures are of this type as well as certain forms of interaction recording (14, 27). The advantages of this technique lie in its impartiality (within the limits of sampling) and in the fact that there is no fixed upper limit on the amount of information that can be collected. The major drawback may well be the tendency to become fascinated by the mechanics of the recording process and to lose sight of the potential significance of the data.

Identification properties

It was noted above that each item of information on group behavior can be identified by three indexes i, j, k, denoting respectively the time or occasion of occurrence, the substantive or functional nature of the behavior, and the persons directly concerned. Distinguishing features among response aggregate measures, then, will be the degree of specificity and the classificatory basis for each of these indexes.

Considering first the time index, i, it is apparent that some behaviors, such as overt motor acts, can be definitely pinpointed on a chronological scale. Other behaviors of social importance (e.g., withholding information of relevance to a group) cannot readily be assigned to a specific time although they may occur during a definite interval. Still other behaviors of a more diffuse sort, such as the revelation of attitudes, are very difficult to place in any way on a time scale. Again, it may not be particularly meaningful to locate behaviors in time at all, but to relate them to some other set of external

events such as a known stimulus input. Still another use of the time index would be to order behaviors in a linear sequence without any reference at all to chronological time. An example of this is Bion's (8) description of group interactions.

The type index, j, may similarly be highly specific as it has been in parent-child interaction studies (2), or it may refer to broad classes of behavior such as "leadership," "supporting behaviors," etc. Many classificatory systems have been proposed for labeling behaviors in small groups: prominent examples are those of Bales (3), Carter *et al.* (11), or Benne and Sheats (7). None of these, however, appears to be of unrestricted usefulness. We still lack the social psychological equivalent of a "periodic table" of behavioral elements or even of the human engineering therbligs.

The agent index k admits of three major variants, again differentiated in terms of specificity. *Directed* measures comprising such data as sociometric ratings and "who-to-whom" conversation protocols require that both the agent and the recipient or object of a behavioral act be stipulated. *Individuated* measures, on the other hand, refer unequivocally to an agent but do not require specification of the recipient. Most role behavior measures come under this heading, the tacit assumption being that the object of the role behavior is irrelevant. *Unitary* data, lastly, do not bear individual tags but refer indifferently to all group members. For some types of behavior (e.g., "forming a group consensus") unitariness is an essential property while for other behaviors it is dictated by metric economy. Perhaps the most important class of unitary data is the so-called "product measures" of group output. Examples are bomb scores for aircrews, trial solutions for problem-solving groups, and the like.

THE DERIVATION OF BEHAVIOR INDEXES

However faithfully a measurement operation manages to reproduce the behavioral events occurring in a group, the record obtained will not be directly useful for prediction purposes. First, the raw data are generally in terms of frequencies in qualitative categories which can be combined or compared only after some relationship among the categories is postulated. Second, response aggregate measures, like all unique events, are of scientific interest only when some order or central tendency is discerned. The search for more stable and quantified measures to characterize a set of response aggregates leads to behavior indexes.

TABLE 1

A HYPOTHETICAL SET OF BEHAVIORAL OBSERVATIONS UNDER VARIOUS STIMULUS CONDITIONS

Stimulus conditions	Persons		
	A	B	C
S_1	b_1	b_2	b_1
S_2	b_2	b_3	b_2
S_3	b_1	b_1	b_1
S_4	b_2	b_3	b_2
S_3	b_4	b_1	b_1
S_5	b_2	b_2	b_4
S_1	b_1	b_3	b_1
S_3	b_1	b_1	b_1
S_2	b_4	b_4	b_2
S_4	b_1	b_3	b_3
S_5	b_1	b_3	b_2
S_1	b_1	b_3	b_1
S_2	b_4	b_2	b_2
S_3	b_1	b_2	b_1
S_2	b_1	b_3	b_1
S_5	b_2	b_3	b_2

The definition of behavior indexes introduced above was that they are summary statistics on behavior sequences which converge over a specified sampling range.[2] The application of this definition suggests a rough and ready typology of such indexes which is most readily presented with the aid of an example.

Table 1 shows a set of hypothetical observations or recordings made on a three person group (composed of "A," "B," and "C") under a variety of stimulus conditions (S_1–S_5), showing the occurrence of one of four types of behavior (b_1–b_4) during the observation period.

In Table 2, the data of Table 1 are summarized in three essentially different ways. The first summarization represents *action* indexes. These are statistical statements[3]

[2] This is analogous to a "consistent" statistic (20) except that it refers to a finite and never completely homogeneous population.

[3] Here, and in the following discussion, we

TABLE 2
CONTINGENCY TABLES ILLUSTRATING VARIOUS TYPES OF BEHAVIOR INDEX

Action indexes

	b_1	b_2	b_3	b_4
A	9	4	0	3
B	3	4	8	1
C	8	6	1	1

Reaction indexes

	b_1	b_2	b_3	b_4
S_1	6	1	2	0
S_2	2	5	2	3
S_3	10	1	0	1
S_4	1	2	3	0
S_5	1	5	2	1

Interaction indexes

		Person B				Person C			
		b_1	b_2	b_3	b_4	b_1	b_2	b_3	b_4
Person A	b_1	2	2	5	0	7	1	1	0
	b_2	0	1	3	0	0	3	0	1
	b_3	0	0	0	0	0	0	0	0
	b_4	1	1	0	1	1	2	0	0

that may be made concerning group behavior which presumably hold over a representative sampling of stimuli within a situation. Examples of such statements in the present case might be "b_1 and b_2 are relatively more salient behaviors than b_3 and b_4"; "Person B is the prime agent for b_3"; "There is relatively little distributional structuring of these behaviors, i.e., one person is almost as likely to perform a given behavior as another," etc. Note that the third statement is independent of content in the sense that the meanings of categories b_1–b_4 are immaterial. This may be true of many important behavior indexes.

The second summarization is representative of *reaction* indexes. These are statements that hold true when the stimulus situation is specified. Examples of such statements in the present case would be: "b_1 is the typical reaction to S_3"; or, "stimulus-response specificity is low for this group." The latter statement, again, is divorced from category content, except insofar as definition of categories may alter the degree of their relationship with the stimulus situation.

The third summarization illustrates *interaction* indexes. These are statistical statements about various behaviors which hold when the prior or concurrent performance of other behaviors is stipulated. As such, they reflect on the general structure of behavioral relationships within a group. Examples of interaction statements would be: "performance of b_1 by person A elicits the performance of b_1 and b_3 by person B and C"; or "A and C interact more closely than do A and B."

It will be noted that the suggested *action* indexes are generally of a sort that might be obtained as marginal totals from the other two index types (all the entries in the illustrative examples may be obtained from the row and column sums for the interaction example). If this throws doubt on the usefulness of action indexes as a separate class, the answer lies in an appeal to the original definition: *action* indexes are statistics that converge over a broad enough range of stimulus conditions so that further subclassification produces diminishing returns. To use a familiar analogy, the within-category variance of a set of measures is likely to decrease as the number of categories is increased but the standard error of individual cell means may *increase* because of decreasing cell numbers. Action indexes, then, are typified by such broad properties of groups as their *raison d'etre*, e.g., "discussion" groups or ladies' bridge groups are likely to engage in conversation whatever may be happening in the outside world.

This typology may be further extended by considering more complex contingency relationships among the i, j, k subscripts, but the principal point of this section seems to be adequately illustrated by the foregoing.

Whatever the particular merits of this classificatory scheme, it is evident that there is a vast untapped area of group behavior indexes. With the advent of high-speed computer equipment permitting such operations as autocorrelation and multiple sorting on a grand scale, there will

ignore the problem of confirming these statements. Conventional techniques such as chi square, analysis of variance, correlation, and autocorrelation, are available for this purpose.

soon be little excuse for summarizing data in terms of gross frequencies provided, of course, that further useful information lies in more sophisticated indexes.

ENDOGENES AS EXPLANATORY CONSTRUCTS

It is most unlikely that any a priori analysis—even if informed by an intensive scrutiny of existing research—could achieve a definitive listing of the true well-springs of group behavior. Except in the broadest outline, the entities or constructs which we refer to as endogenes must be thought of as objectives rather than as starting points for investigation. At the same time, completely blind research—of the "measure-everything-and-factor-analyze-it" school—seems a drab and unpromising method of attack.[4] The most expeditious way of discovering such quantities is, without doubt, by a continued process of successive approximation. Various forms of intervening variables may be postulated and situations devised which test the alleged invariance of these quantities.

Here we shall readily admit that hunches and clinical insights are indispensable for progress in this area. Our present concern will be not with the content or source of these intuitions but with the "rules of the game" that might determine their application to the analysis of groups. We shall propose five desiderata that might govern the selection of endogenes.

First, endogenes should be demonstrably *invariant* as suggested in the introduction. That is, they should not reflect environmental changes except through the mediation of customary learning and extinction processes with a respectable degree of lag. As an obvious example, no proposed endogene would be very useful for predicting group reaction to stress if it was itself susceptible to stress reactions.

Second, endogenes should be defined in such a way that they are *distributed* over all aspects of group behavior and over all group members. This is most readily illustrated by an example. Paternal leadership presumably refers to the behavior of a single individual—the leader—but it is a general condition of group membership insofar as it characterizes the relationship between each person and the leader. If it happens that, for some group members, the leader's paternalism is received with resentment or amusement, then some more general concept would be required to encompass the general group characteristic.

A third desideratum of endogenes is that they should be *non-hereditary*. By this is meant that once the value of an endogene is determined for a particular group, this value is independent of its genetic origin. As an example, it is claimed that "cohesiveness" has virtually the same magical effects whether it originates in positive interpersonal attractions or intrinsic group goal values (cf. 18, 29). If so, a numerical value of cohesiveness should be valid in prediction equations of the sort introduced above whatever the antecedent or etiological conditions may have been.

Fourth, endogenes should be *syntactic*, that is, they should combine with, and be related to, other terms according to definite mathematical rules. This means, for example, that an endogene which is expressed as a real number in a continuum is somewhat more useful than are endogenes that can only be expressed in qualitative typologies. It suggests further that the use of more recondite mathematical systems, such as vector and tensor analysis, might yield increasing dividends.

A final desideratum for endogenes is *non-emergence*. By this is meant that the endogene should be identified as a definite function of the personality characteristics and behavioral propensities of the individuals making up the group. Of course such a function need not be simple and, as suggested above, there may be several distinctly different antecedents for the same endogene. There is a strong tendency, though, to assume that any quantity which is not a simple average of ingredient factors

[4] It is not intended to disparage such factor analytic studies as those of Borgatta (9), Carter, *et al.* (11), and Hemphill and Westie (19). The congruence of results obtained on these studies is in itself an encouraging sign. The factors obtained, however, are more closely related to the previous category of *action indexes* than they are to *endogenes* as the term is used here.

is emergent or quasi-emergent. This is a far from necessary conclusion since, in fact, we have scarcely scratched the surface of intermediate composition functions.

DISCUSSION

To recapitulate briefly the argument of the foregoing sections, it is asserted first that group measurement is best understood as an essential aspect of the general prediction problem. From this standpoint there are three rather different classes of group measures in addition to such situational measures as may be obtained. The clearest basis of distinction among these measures seems to lie in the degree of invariance which attaches (at least by hypothesis) to the three classes: *response aggregate* measures are completely the pawns of circumstance and represent the raw data; *behavior indexes* are invariant over a specific sampling range; *endogenes* change only when the group itself changes in character.

From an epistemological point of view, response aggregate measures are the firm substance of the science. Behavior indexes may be tested by statistical means but their reality as constructs is always subject to the vicissitudes of sampling. Endogenes, at least at present, exist only on the sufferance of the individual psychologist. A really grim behaviorist may see no need for them at all.

We are faced, therefore, with an unfortunate inverse relationship between the potential predictive power of various measures and the degree of certainty that attaches to the determination of those measures. This situation appears to dictate a certain strategy of approach to problems of measurement and prediction which will be briefly described.

At the rather primitive level of contemporary social psychology, predictions must be of a modest sort. For example, if a fair amount is known about a test situation, S_o, then it is possible to predict, say, the ordering of a number of groups on certain behavior indexes under presumably similar conditions S_p. Here, no attempt is made to predict absolute behavioral levels but only relative frequencies of pertinent behaviors among groups. At the other end of the nomothetic-ideographic spectrum, it is now possible to forecast with some confidence the continuation of measured interpersonal attitudes in individual groups over a diversity of situations (6, 16). In the former case it is assumed that the transformation from X to Y, and the parallel transformation from S_o to S_p, are "order preserving" transformations: in the latter case F_a is equated with F_b and it is assumed that situational parameters are of comparatively minor importance. In both cases, the existence of endogenes remains an article of faith of no immediate operational significance.

A second stage of progress in the prediction problem should see the emergence of definite leads as to what forms the E values may take and increasing sophistication in the treatment of functional relationships of the sort embodied in equations 1a and 1b above. This will permit some degree of prediction to a variety of possible prediction situations from a variety of observation situations—granting some progress in the definition of relevant situational variables.

A final foreseeable stage of progress would involve the gradual elimination of the semantic difficulties involved in the use of intervening variables such as the E_s. In brief, it would consist in the development of S_o situations which tapped variations in the E_s as directly as possible. Thus, the endogenous quantities might finally appear only in forms which could be given direct meaning in observational operations.[5]

The foregoing analysis has been concerned chiefly with what might be called the "psychological" properties of groups, that is, intrinsic group properties which might clearly differentiate the behavior of various groups under similar circumstances. There are, however, a number of ways of describing groups "sociologically," on the basis of the external circumstances themselves. Thus, we can differentiate groups in terms of the mode of original recruitment, the purposes of group activity, the spatial or functional

[5] This seems to be similar to the evolutionary process followed by more advanced branches of psychology and other sciences. That is, there is first a recognition of regularities and uniformities, followed by the attribution of these to sufficient internal properties of the systems being studied, and, finally, the development of "canonical" measures which correspond closely to the postulated intervening variables (cf. Weyl [31]).

distribution of group members (5, 13, 25), or the superordinate structure under which the group operates (17). A great many "predictions" can be based on these labels without any direct observation of the group itself.

It appears to the author that this line of investigation reduces to the task of describing meaningful situational parameters (S_o and S_p) and can only achieve its ultimate objectives if it is closely coordinated with the psychological approach which the foregoing discussion has treated. In other words, it is equally impossible to identify group characteristics out of their situational context and to identify meaningful environmental parameters without tracing their effects on group behavior.

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"DEMOCRATIZATION" AND AUTHORITY IN A THERAPEUTIC COMMUNITY[1]

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The phrase "doctor's orders" reflects the important aspect of authority in therapeutic situations. Yet the avoidance of coercive authority has become an important element in the relation between the psychotherapist and his patient. How can the two aspects of therapy be reconciled, especially in a hospital, where considerable centralization of authority is necessary just to run the hospital?

In the attempt to solve this problem, the notion of a "therapeutic community" has evolved, an experimental form of organization aimed at "democratizing the authority system of the hospital."

PSYCHIATRIC treatment is carried on in a variety of therapist-patient situations. The transactions are roughly divided into those of physical treatments (drugs, surgery, electroshock) and psychotherapy (purely psychological impact). The contexts range from the two-person relationship in a private consulting room through the complex social system of a modern hospital. In all these situations authority is an important aspect of the treatment, but it is important in different ways. Where the method is physical,[2] the authority relationship is secondary in producing the therapeutic effect; where psychological, the authority dimension is of focal importance.

An important element of psychotherapy deriving from the psychoanalytic, nondirective, and kindred orientations is the idea that the therapist should not use his authority coercively with the patient.[3] Where similar theoretical orientations have guided group psychotherapy the same kind of preference has prevailed (1, 3, 4). This ideal can more easily be approximated in private practice. However, in large institutions where complex interpersonal relationships must be coordinated to allow the institution to function, a greater degree of regulation and authoritative action becomes necessary. The extreme case of this is in mental hospitals that forcibly restrain certified patients, but it holds in some degree for all hospitals.

Where a hospital uses this kind of psychotherapy as a major method there is a "built in" dilemma. To give treatment, it is believed that the use of power must be avoided; to run a hospital it must be used in some degree. The principal attempt to analyze one kind of clinical solution to this dilemma has been the work of Stanton and Schwartz (14). The pattern described there for Chestnut Lodge typifies hospitals in which psychoanalysis is the main treatment method (8, 9). Their solution is to create separate administrative and therapeutic roles within the medical staff, so that administrative physicians can attend to activities with power

[1] This is part of a larger research project for the Belmont Hospital Social Rehabilitation Unit, Dr. Maxwell Jones, Director. Thanks are due to the Medical Research Council and Nuffield Foundation who have financed the work and to members of the Unit's clinical and research teams with whom discussions have taken place. This paper was first prepared for the American Anthropological Association meeting, December 1956.

[2] I.e., where research has indicated that physical processes rather than psychological concomitants are the effective elements.

[3] For an elaboration of this position in its more general context, see Ref. 13.

implications and therapists can carry on treatment unhampered by such duties. The hazards of such a system seem to include the possibility of unrecognized splits between the two kinds of figures who have segmental influence over individual patients. These splits seem associated with clinical disturbance in the patients concerned (14).

The Belmont Hospital Social Rehabilitation Unit, applying modified psychoanalytic ideas in the context of a "therapeutic community" (5, 6, 12) exemplifies another kind of solution to the same dilemma. It has experimented with "democratization" of its authority system.[4] The attempt is made to reduce as far as possible the authoritative aspect of administration and to diffuse decision-making responsibility as well as therapeutic activity to staff and patients throughout the social system. The purpose of this paper is to examine some aspects of this organization of authority from two points of view:

a. to see how far it has been possible to achieve the ideals of "democratization" of authority within this hospital structure,

b. to touch on some therapeutic implications of this form of democratization.

The Hospital

The Belmont Social Rehabilitation Unit is an experimental section of a neurosis center serving all England under the National Health Service. It has been set up under the direction of Dr. Maxwell Jones to explore the therapeutic community approach to psychotherapy. It serves about 100 patients with various forms of personality disorders (but excluding psychotics or mental defectives) as in-patients for periods up to a year. The staff is made up of a conventional psychiatric core (psychiatrists, social worker, nurses, clinical psychologist) and a peripheral staff untrained in psychiatric hospital methods but oriented to community therapy.

[4] The term *democracy* is used in this paper to mean, as it does in common usage in the Unit, participation in administration and treatment of all ranks and roles within the social organization. It does *not*, as in the political concept, imply that those having formal authority are given it by those over whom they can exercise power. See Ref. 7, 15.

The Unit developed out of wartime experiences which highlighted, for example, the therapeutic value of group discussions, of using personally admirable though untrained nursing staff, and of socio-environmental influences. In the postwar period, the core staff turned its attention to broader social problems and announced that these methods had a relevance for the reform of mental hospitals generally and the treatment of chronic social misfits.

It sought, with the idea of democratization, to broaden the scope of potential therapeutic influence within the Unit. Hospitals' formal hierarchy of roles, designated by named positions filled according to technical qualifications, governs the allocation of formal authority and responsibility. This system affects such details as the differential distribution of wages, use of facilities, access to professional channels of referral and recruitment. But there are other "informal" capacities that have become more important in governing the flow of interpersonal influence on which community therapy is based. These include intelligence, perceptiveness and sensitivity to the intrapsychic processes of other individuals, group leadership skills, and access to informal interaction situations. Officially, it is assumed that the effective distribution of these informal capacities will be congruent with the formal role structure. In contrast, the Unit's assumption is that a more effective harnessing of the treatment potentials within a therapeutic community could be achieved by "flattening" the hierarchy and "submerging" the formal structure in an equalitarian-democratic system legitimizing the use of informal capacities wherever found in the population.

The treatment ideology with regard to authority

The Unit advocates the flattening of hierarchies. It aims to diffuse authoritative activity through the system by legitimizing interaction among patients. When these collateral influences are exerted by patients congruently with those judged therapeutic by the staff they are termed "constructive"; when incongruently, "destructive." The rationale for this approach may be paraphrased as follows:

Patients of the type treated in the Unit tend to have authority problems (e.g., criminality, job instability, disturbed social relations).

Their present pattern of difficulties is assumed to be derived from early difficulties in interpersonal, especially parental, relationships. These are thought of as based on misconceptions about authority figures who are seen as rejecting or repressive.

It is considered desirable from the point of view of relearning that the patient reproduce, or transfer, his recurring conflicts into the interpersonal life of the Unit where these underlying misconceptions can be analyzed.

In order to enhance relearning rather than simple repetition of difficulties in authority relations, the situation must be one in which patients cannot reasonably attribute malevolent motivation to authority figures. Patients of this type tend to provoke restrictive reactions in people in authority positions and then withdraw feeling angry, rejected, or injured. In practical terms this means that antisocial patients tend to be so contrasuggestive to the efforts of institutional personages that their voluntary participation in treatment is difficult to manage. Diffusing the locus of authoritative activity is felt to diminish this contrasuggestive effect, and to make better use of the positive effects of collateral influence on patients of this type.

In the Unit, there is not only a flattening of the hierarchy but also a blurring of the role structure. It is felt that as formal aspects of the role system become less determinate in dictating type of behavior, persons become more interchangeable within the system. To the extent that this is true, patients can select more freely the kinds of people to whom they tend to relate, and to relate to them in ways dictated by inner compulsions rather than the role prescriptions. As an individual repeats his typical relational difficulties he provides others with data for analysis. Anyone may have knowledge of or become involved in this "acting out" universe, and everyone's participation in the analysis of behavior is authorized.

Such role-taking flexibility is felt to offer patients a chance at the kind of role-rehearsals indispensable for emotional development but defective in their earlier lives.

Universal participation in the analysis of behavior is also thought to improve other qualities of communications. For example, psychiatric staff members tend to be of the middle classes, whereas patients in this setting tend to be of the lower and working classes. Other studies have shown how differences of this kind raise semantic and normative problems and the use of collateral channels offers one approach to dealing with them (10).

Devices for diffusing and collateralizing authority

The staff members who hold actual formal authority in the Unit, use several devices for transferring the exercise of authoritative activity from themselves. Ideally they do not simply shift decision-making responsibility, or arbitrarily abdicate their authority positions, but show the following patterned preferences for other modes of authority functioning:

1. Decreasing the use of formal differentiating symbols: e.g., special titles, uniforms, forms of address, special access to facilities, and other status prerogatives.

2. Displacement onto regulations: "the report book says . . . ," "the 9.00 rule says . . . ." However, in accordance with the ideal of permissiveness, the number of such explicit regulations is kept as low as possible.

3. Displacement onto a collective, or nonempirical entity: "the group says . . . ," "the culture says . . . ," "the community decided . . . ." Authoritative prerogatives are given to groups, within which staff and patients participate together.

4. Displacement onto another time period: "traditionally (in the past) the patients have wanted . . . ." Locating an authoritative decision thus has a nonempirical quality, since most of the community is composed of transient members.

5. Displacement onto an external (usually higher) authority: "the hospital won't . . . ," "the head of nurses insists . . . ."

6. The selection of personnel who enhance this aim: e.g., personalities that can tolerate

a comparative lack of definition and location of authority without becoming excessively anxious or authoritarian. Also, the system of favoring transient untrained staff at the junior levels enhances the probability that they will orient themselves toward the core staff as a matter of situational dependency rather than formal requirements.

Anomalies in the exercise of authority

The ideal of democratization is approached through the devices mentioned. New members of the Unit community are told to "find their own roles," and to do and say what they feel. This creates an impression of freedom from authoritative restraint. The assertion that "we all need treatment sometimes"[5] blurs even the fundamental issue of who is giving treatment to whom.

However, any observation of social process within the Unit yields the conclusion that the way authority is exercised is highly variable. Senior staff members insist in some meetings that they have "only one voice in community affairs," or that "the group's decision is binding," and on other occasions will discharge patients against group consensus, make major decisions about the handling of patients' affairs, inaugurate or terminate group events, and so on. The ideal-real gap seems to undergo fluctuations that are linked to the levels of tension and overall state of organization in the Unit as a whole (11).

Factors governing differential functioning of authority

Some systematic influences that appear to determine and set limits to the kind of authoritative action taken appear to be:

A. Factors Internal to the Unit:

1. *Conflicting values: Other* ideal principles may cross-cut those of democratization. (E.g., if a majority of a group were to decide to go for a walk in the country instead of to work-therapy shops, the staff minority would tend to reassert their overriding influence to contravene the group decision on the grounds that such a decision was "unrealistic" in terms of rehabilitation goals.)

2. *Differential professional or administrative responsibility: Formal* role differences within the Unit may come into play. (E.g., if a doctor were to judge that a given patient were on the verge of a psychotic breakdown, he would tend to take steps to place him in a more protected environment regardless of group consensus which might oppose sending the patient away for a host of nontechnical reasons.)

3. *Personality factors:* Authoritative action may be taken as a defense against personal anxiety. (E.g., if a doctor were away leaving two nurses in a group of patients ordinarily taken by the doctor, one might have a lower anxiety-tolerance threshold and act authoritatively before the other felt it necessary.)

4. *Group organizational factors:* Staff judgments may require the restoration of a better state of group equilibrium. (E.g., if enough patients were passive, confused, or disturbed, the staff might act authoritatively to send some away, give support to others, or to give direction and instructions to the groups. They would say things like "the patients are finding the responsibility more than they can manage," or "they are being too destructive," or "punitive," or "scapegoating.") Sometimes, in such circumstances, staff members are split on the appropriate course of action, and these splits seem (as other studies have also indicated) to be reflected in disturbed behavior by patients who have invested the staff members on both sides of the split with heightened emotions transferred from childhood relations. In extreme cases, such patients are called "psychotic," and sent to mental hospitals for other forms of treatment.

5. *Situational factors:* Where emergency action is needed and the staff bear some kind of special responsibility (e.g., in the case of a fire).

B. Factors External to the Unit:

1. *Real or anticipated conflict with higher authorities:* The hospital organization or larger professional organizations may become involved. (E.g., if the Unit as a whole

[5] The extreme version of this, favored by one doctor is "we are all patients here together."

were to consider keeping a patient who had offended hospital officials and whose discharge had been ordered on disciplinary grounds, regardless of internal consensus, or therapeutic prognosis, the patient would have to be discharged.)

2. *Preventing potential conflict with society:* Cultural values of the surrounding society may be involved. (E.g., if the Unit's permissiveness toward "acting out" were to lead to excessive sexual promiscuity and cohabitation in the wards, action might be taken by the staff either because of cultural values held by individual staff members or to prevent conflict with external society holding these values, prior to any group consensus or clear medical basis for action.)

3. *Results of actual conflict with society:* Representatives of external authority may actually intervene. (E.g., if a patient were to commit a grave crime and be traced to the Unit by the police, staff members would cooperate with the law regardless of internal group consensus or therapeutic prognosis on the case.)

Conclusion

Study of social processes within the Unit yields the conclusions that the ideal principle of democratization is put into practice irregularly because of cross-cutting determinants. In addition it seems irregularly associated with therapeutic success. Sometimes, therapeutic effects appear to accompany "democratic" functioning, and sometimes the staff seem to evoke contrasting principles to produce desired results. On still other occasions the adherence to their democratic principles seems to be accompanied by therapeutic failure.

The Unit, like other institutional communities, is subject to social processes that are somewhat autonomous, or independent of the specialized goals of the organization. Some of these processes are congruent with the therapeutic goals, and others are less so. Ideally, the congruence should be maximized. In the Unit it has been noted that the therapeutic ideal of democratization is inconsistent with the institutional structure on which it is imposed, and this is a factor precluding absolute congruence. This factor alone tends to produce an inconsistency of functioning which is associated with undesirable consequences. On the other hand, the very existence of the ideal of continually increasing staff-patient equal participation in all aspects of hospital life promotes internal solidarity in the face of the basic cleavage in institutions of this kind between staff and patients.[6]

Apart from practicability, though, perhaps the most important question at this stage is how far such "democratic" processes are therapeutic. We need to know more about what kinds of patients benefit from what kinds and degrees of democratization in conjunction with what other features of a therapeutic community. The model for authority organization described by Stanton and Schwartz seems to carry with it the potential hazard of staff splits accompanied by patient disturbances. The model described for Belmont seems to carry with it hazards in the oscillatory processes of involvement and recession from optimal therapeutic relationships, though the oscillations themselves may be managed to therapeutic advantage (11). Both systems seem partly to implement a particular treatment conception, and each seems to carry with it certain undesirable features in terms of its own conception. The undesirable concomitants can, in each case, be partly remedied when they are brought into awareness. In the Chestnut Lodge situation, for example, new channels for maintaining staff unity are prescribed (2). In the Belmont situation, too, certain modifications of formal structure seem to counteract the undesirable by-products of the oscillatory process[7]. However, new developments in therapeutic communities will need to consider in detail not only how to handle un-

[6] The extreme version of this ideal is the image of a sort of "withering away of the staff" with patient-run treatment centers and society at large handling its own psychiatric casualties as a great therapeutic community.

[7] For example, the formal relationships with external administrative authorities are undergoing revision with the goal in mind of reducing this determinant of differential authority functioning. Also, ideological themes are under examination in order to create a better awareness of their areas of overlap and conflict. Greater clarification of these problems will be reported in later publications.

desirable by-products of established authority structures, but how to construct formal role systems in which the functioning of authority is more closely geared to treatment goals and methods.

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The philosophic trend of modern scientific thought differs markedly from the views of thirty years ago. Can we guarantee that the next thirty years will not see another revolution, perhaps even a complete reaction? We may certainly expect great changes, and by that time many things will appear in a new aspect. That is one of the difficulties in the relations of science and philosophy; that is why the scientist as a rule pays so little heed to the philosophical implications of his own discoveries. By dogged endeavour he is slowly and tortuously advancing to purer and purer truth; but his ideas seem to zigzag in a manner most disconcerting to the onlooker. Scientific discovery is like the fitting together of the pieces of a great jig-saw puzzle; a revolution of science does not mean that the pieces already arranged and interlocked have to be dispersed; it means that in fitting on fresh pieces we have had to revise our impression of what the puzzle-picture is going to be like.

—A. S. EDDINGTON in *The Nature of the Physical World*

PROBLEMS IN THE INSTITUTIONALIZATION OF HIGHER EDUCATION; AN ANALYSIS BASED ON HISTORICAL MATERIALS[1]

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Clues to the answers for such questions as the best preparation of qualified men for creative social science research, the extent to which the past development of natural science can be utilized as a guide to the future development of social science, the optimum educational organization, the proper balance between quantity and quality factors, the dichotomy between teaching and research, the balance between theory and general learning versus practical learning are sought in the dynamic interpretation of the historical process of education, and the controlling factors which help bring about educational change, both institutional and functional, are discussed. For example, reasons are suggested for the dominance of the department in universities and for the high or low prestige of the institutions.

OBJECTIVES

THE goals of this seminar have been: to find and identify the crises and turning points in the history of higher education; to examine past efforts at establishing adequate research training in the social sciences; to identify and separate the significant aspects of the entire problem; and to examine the particular factors which have, over the decades, blocked a rational solution of it.

[1]Based on a presentation at the Center for Advanced Study in the Behavioral Sciences, August 9, 1955, in the seminar on "Training in Social Sciences," conducted by Drs. Paul Lazarsfeld, Director, Bureau of Applied Social Research, Columbia University, and W. H. Cowley, Professor of Higher Education, Stanford University. As the title indicates, the author has drawn heavily on the rich historical materials collected by Dr. Bernhard J. Stern for the Columbia research project and Dr. Cowley for his classes in higher education, as background reading for the seminar. Dr. Stern's preliminary document was made available in mimeograph form by Dr. Lazarsfeld. Since Stern's untimely death will delay publication of the volume for some time, page references here are to this mimeographed version. Dr. Cowley also made available his extensive mimeographed manuscript; it is to be hoped that this also will be published. The author has attempted in this presentation to organize these into a coherent picture leading to some working propositions.

It is asking much to expect rational solutions in human affairs, especially in view of the notorious divergence, from a priori expectations, of the actual consequences of action. This is well exemplified in the case of higher education by the totally unforeseen impetus given to it by the Land Grant Act. Ross (4) (as quoted by Stern, page 203) says:

> An appreciation of the act (The federal Morrell Act of 1862 which gave public aid for agricultural and industrial education) on the basis of the greatness of the institutions to which it was to contribute would have been wholly anachronous. The original law, while offering the possibility of strengthening existing projects in industrial education and of founding new ones, in itself assured nothing. The acreage or scrip, of uncertain value, was to be secured on conditions most uncertain of fulfillment. Contrary to later assumptions regarding the immediate influence, there was real doubt as to whether the provisions would be generally accepted. So dependable a supporter as Greeley sought to discount expectation of limited utilization by the assurance that the act would be justified if it led to the establishment of not more than five true industrial colleges. Superlative encomiums of the measure as the "greatest act in the history of higher education" would find no contemporary substantiation, in professional assurance or popular recognition. The possibilities of this plan of national aid were only to be manifested in the course of time as the states utilized

it in accord with their particular conditions and needs. . . .

More specifically, the Stern materials were collected to help answer such questions as: How can an adequate program of research training in the social sciences be fitted into the university structure? How can sufficiently flexible relations be obtained and maintained between the relevant disciplines? How can the curriculum be kept modern and manageable? And how can one define and create adequate work materials, such as case studies, and insure effective field work, research center experience, and the like? The presumption was that clues as to valid answers to these questions could be gleaned from the evidence of history.

Let me now recast the above in the language of a specific research investigation. There is a definite problem: How can qualified men be prepared for creative research in social science? There is a primary, and a subsidiary, hypothesis: Certain institutional structures and teaching processes are better than others for such preparation; and these desirable ones can be identified and promoted. Relevant evidence is to be sought in the history of higher education in general and especially from examination of the growth of the agencies that foster creative research in the natural sciences, the latter being a couple of centuries ahead of the social sciences in their development. Finally, we would hope that some conclusions, even tentative ones, would be suggested by this evidence and that these would guide future experiment and action in the creation of appropriate agencies in the social science area.

On the basis of the data presented by Stern and by Cowley plus further consideration of the over-all picture, some of the factors that have operated to aid or hinder the development of institutions will be identified, a specific case or two explored, and a few generalizations for further testing suggested. But first it is necessary to point out some problems and limitations in the use of these historical materials, rich though they be.

HISTORY AND METHODOLOGICAL PROBLEMS

Historical facts are of necessity anecdotal. In the absence of adequate sampling procedure, or knowledge of the total universe from which the particular examples have been excerpted, it is never possible to weigh the pro and con evidence with any precision or to draw conclusions with assurance. It would be a nice, perhaps a worthwhile, study to examine the whole universe of institutions in a given time and area, taking care to include the mediocre ones and the failures which ordinarily do not come down in the anecdotal material, and to do so with some attempt at quantification of the significant factors. If, for all the colleges and universities started in the United States between 1860 and 1890 one could obtain quantitative information on the funds available, the number of students, the library holdings, the actual use of books, the kinds of purchases made for teaching and for research, the questions posited in course examinations, and the like, and if the degree of success of the institution and of its graduates showed some clear relation to one or another of these factors, then and only then might firm conclusions be justified. The mere fact that informed and sincere men can continue over the decades to espouse opposing interpretations of history and opposing programs of action merely proves that the arguments on both sides are good but not compelling and that the evidence is inadequate to give a decisive resolution. When the evidence is sufficient the argument ends; the statement that has impressed me most in this entire seminar is that President Lowell abruptly altered the attitude of Harvard undergraduates, who disdained a grade above "a gentleman's C," by offering clear evidence that undergraduate grades correlated highly with career success.

A second important question is the extent to which the past development of natural science can safely be projected as a guide to the future development of social science. It might be argued, for example, that natural seience very early gave practical proof of its utility and that the resultant prompt and great demand for men and their products

would not be paralleled in social science. It is my impression that the demand preceded the product in the case of natural science; that the discovery of the new world, the heroic growth in exploration and navigation needs, the swelling mercantilism, and other important changes of the parturient 16th and 17th centuries, were more the antecedents than the consequences of natural science. I suspect, further, that the present need for comparable fruits in the social or behavioral sciences are as pressing as were ever the needs for insight and invention in the material sphere. But I am no expert on these matters, nor can I judge how soon the social sciences will burgeon with useful products. Perhaps Conant's estimate of 150 years between seeding and fruiting in the natural sciences would apply; but I suspect—perhaps only because I so much hope—that the lag will be much less.

Nor is this the occasion to ask the more searching question, whether the overwhelming success of the natural sciences has been entirely good for mankind. Did man gain the earth only to lose his own soul? Certainly the fine arts and humanities in our present universities are undernourished as compared to the sciences; but whether they are less well off than they would have been if science had not exploded the small colleges of the past into the great universities of today, is a more difficult question. In any event, if the successes of the natural sciences can be regarded as a questionable blessing, how much more open to query would be success in the behavioral sciences, with the expected growth in control of human action and thought?

A number of clues have been suggested as to the elements that must be involved in any solution of our problem. The issues that have formed interweaving threads, and on which explicit decisions must be taken, include at least the following: What is the optimum organization; should institutions (institutes?) be part of universities, be separated from them, or be attached to them more or less loosely? What is the proper balance between the quantity and the quality factor as regards the resources and faculty and especially the students; is it better to have a few quality students or many mine-run ones? Should the dichotomy between teaching and research be encouraged or opposed and, in either case, what should be the relative emphasis on each? Similarly, what of the balance between theory or general learning versus practical or particular learning or, in a slightly different breakdown, the balance between pure and applied; and is "supported" a fair synonym for "applied"? Further, is the real issue in these cases the significance of the problem being undertaken rather than its orientation toward general or particular, pure or supported? Finally, individual institutes enjoy a status, compared to the more established organizations, which varies from quite high to very low; what are the factors that contribute to the status level of an institute?

Finally, in this mood of critical analysis, come the questions whether the development of higher education and its institutions is any different from the general evolution of culture and the social institutions that embody it and whether anything can be done about it. As developed elsewhere (1), I strongly suspect that natural selection dominates social, as well as biological, evolution and that, with a certain amount of chance selection of the particular case due to the accidents of "drift," it determines survival or extinction. But perhaps in social evolution there exists—what has always so far failed of proof in the biological case—a kind of directive factor. Perhaps the new idea, which generates new social entities as the gene generates new biological individuals, is somehow brought about by a need for it; perhaps, indeed, necessity is the mother of invention. But I am still inclined to look to chance variation, as clearly put by Mann (3) (Stern p. 177).

It is difficult to interpret the interplay that has been going on among industry, science, and engineering. At the close of the Civil War science had but scant recognition either in educational institutions or among the masses of the people. Now it has assumed a commanding position because of the transformations it has wrought in the daily life of every one thru its varied and fruitful inventions. In this development there has been no regular procedure, no well-defined organization. It has been a matter of independent action and individual effort. Sometimes it was the college

professor of science, pure or applied, sometimes it was the inventor or the professional engineer, and sometimes it was the manufacturing industry that took the initiative, conceived the new idea, or made the new discovery, and sought the assistance of the others in realizing it in practice. Now evidences are multiplying to show that the time has come for a clearer definition of the relations among research, instruction, engineering practice, and industrial production. How to coördinate these elements most effectively is a large and pressing problem.

So much for analyses and anxieties. Despite these warnings of error and exhibitions of uncertainty, I suggest the following propositions bearing on the appearance and fate of new institutions.

THE ONTOGENY OF INSTITUTIONS

Societies steadily evolve. Whether or not one believes in "progress," the change tends to be unidirectional and toward increasing complexity. At each stage, some sort of structure or organ serves the current functional need; but as functional needs change, the structures that serve them lag behind. The more firmly established a structure, the more completely it is elaborated into an ongoing institution, the greater is this lag and the more strained is the discontinuity between current need and obsolescent instrument. The more structured a given organ, the harder it is to change or substitute it—in social no less than in biological systems. For this reason, if no other, universities would be resistant to anything really new arising in their social milieu.

The needs must be met, however, when they become sufficiently intense; and if they are eschewed by the established institutions the anlages of new structures will be created by the amorphous efforts of outsiders. These outsiders, almost by definition, are likely to come from the artisan rather than from the professional level and are likely to be more second than first rate. Again, specifically for universities, the new content area, originally scorned because of its intellectual limitations, is now completely rejected because of the unworthiness of its adherents. Let me exemplify before proceeding.

The present institutions of higher education originated from informal bands of students, eager to improve their professional status and knowledge. Theology, medicine, and law slowly became established, in time spawned the intellectual disciplines, with them came to dominate the universities, and now reject the present applied arts in all their new manifestations. Modern experimental science was itself an early intruder into the halls of professional disputation. From the time of the Greeks onward, hand work was demeaning and when eccentric savants wished to labor with objects, they had to find their labor-atories outside the universities. Physical science was nurtured in the scientific societies of the 17th century and was long an extra university activity. When the Ecole Polytechnique was established in Paris, at the close of the 18th century, the aim was primarily military engineering; the creation of science departments and laboratories in universities was an unexpected consequence. Similarly, science came to the United States primarily through the agricultural and engineering schools to meet the needs of the new frontier, the expanding commerce and navigation and industry, and the attendant need for civil engineering. Nor should it be forgotten that the advance of pure science has often depended on the development of applied science; until the pump had been invented, Boyle could not have established the pressure-volume law for gases; nor could Harvey have easily conceived of the heart as a pump and so clarified the circulation of the blood. Thus, pure science grew from the problems and with the tools presented it by society as a whole and the professions in particular. It thus penetrated the existing institutions with difficulty. The first school of science in this country, now the Sheffield Scientific School of Yale, born a scant century ago, long supported itself with fees from industry and, for an even longer time, was treated as a disreputable relative by the elder colleges of the University.

Returning from this particularization to the broader sweep of institutional evolution, we left the new activities slowly taking form in the hands of outsiders. Sometimes at the start but more often later, the *leader* emerges. He may be directly responsible for a new master concept that illumines the

whole field, he may successfully synthesize or formulate what has already been thought and said, or he may be the vigorous crusader who brings fire to what is already illuminated (one might consider the example of Darwin and Huxley in these several roles); but whatever his particular contribution, the new movement rallies around and crystallizes upon him. His followers, the outcasts and repudiated, become a group of zealots, overdedicated to their cause, and deeply committed to raising the prestige and acceptance of their subject and themselves.

Sooner or later the conflict between the new and the old resolves itself and the parvenu succeeds or dies. In the great majority of cases, the outcome is failure. The new idea in culture, as the new genetic mutation in organisms, in the great preponderance of cases leads to bad consequences; and the innovation suffers a well-deserved extinction. Phrenology may serve as an older and semi-serious instance; dianetics, as a recent and trivial one. A limited number of the innovations succeed, despite logically untenable positions, because they fulfil emotional or other implicit needs related to the Zeitgeist or for entirely irrelevant reasons. Chiropractic, as a medical cult, and Christian Science, as a religious one, are examples of successes that I would judge as rationally undeserved. Finally, there are the extremely few successes of important and valid innovations that do deserve to succeed. These are the seeds from which spring great social changes—Darwin and evolution, Freud and psychoanalysis, Montessori and the kindergarten, Florence Nightingale and the nursing profession.

Again a brief diversion in our area of particular interest. A great many technical schools exist today, over the full range of good to bad. I can think of no first rate one whose educational program does not stress the teaching of theory more than of practice. One wonders whether this devotion to theory led to the development of great teachers and wise graduates, or whether a farsighted and imaginative faculty generated both the good institution and the attention to theory. At least the converse view, that practical men are best trained by practical experience, has led to some disastrous results. The early land-grant colleges did demand that their students work in factories or in fields and get "Real Experience" on the job. Today such manual experience, if a part of the formal schooling, is obtained in high schools, where it perhaps belongs, and is not blocking the proper aims of higher education. Again hear Ross (Stern, p. 209):

> After long and persistent trial the system (of required labor) failed to realize any of these aims satisfactorily. Educative work was not remunerative, and the labor for which payment could be afforded was not instructive; gymnastics and athletics provided more appropriate and effective facilities for physical exercise and training; mechanization in industry and agriculture made manual skill and endurance decreasingly significant; and the adoption of the laboratory method in applied science superseded any possible instructional benefits. The emphasis given to this feature of land-grant instruction—the time, effort, and money expended upon it—while in line with popular desire, served to discredit the colleges in educational and scientific standing and thus to retard the public service that they were seeking to render.

Returning now to the main theme of institutional development, the breakthrough of the new, the successful establishment of a fresh orientation or institution, is most likely to occur at a time of social crisis and is commonly associated with the great strains of a war. The habitual is more easily discarded under stressful than placid conditions and, more important than changes in individuals, there is change of individuals in important roles. Younger men, who entertained "advanced ideas" but had been impotent, then rise rapidly to power positions and make possible a jump or saltation, by a generation, in the ideas accepted and the strategies followed. I suspect that social change in general comes about by such saltations—older persons rarely change the basic attitudes of their youth but are in time replaced by another generation which activates the ideas new in its own youth—and that crises merely accelerate the tempo of replacement. Ordinarily, the commitment of the conservative is more clear-cut than that of the innovator. The conservative knows what he has, likes it as it is, and is willing to fight to retain this concrete good; the innovator has only the hope that the change he favors will lead to a more satisfactory situation than

the present one. The innovator, then, if he is to generate any effective pressure in himself and others, must develop an especial intensity of commitment or belief or dedication, or even of zealotism.

The new institution is thus likely to get over the hump at a time of social upheaval and with the aid of young and dedicated men who obtain authority. It is hardly surprising that, under these conditions of unconventional leadership, many liberal changes are made contemporaneously and in relation to each other.

The wave of the present passes, the social waters settle, and a new institution is firmly and successfully imbedded in the social structure. Then the period of decay sets in. Success attracts opportunists, and the dedicated creators are soon overwhelmed by those who go through the forms but smother the spirit. They debase the content and vulgarize the output, and these changes lead to consternation among the pioneers and faithful. Barriers are erected against the mob, formal rules and hurdles are set up; but these are in turn circumvented and overcome. Thus, choosing medicine as an illustration, the route to professional acceptance was, first, through the informal and personal apprenticeship, then the proprietary school, then, as abuses grew, through the university degree. Even this did not suffice and government stepped in with a formal license requirement. Knowledge and skill demands continued to grow, and the profession itself introduced the further hurdles of a specialty board examination and, just now appearing as a cap on this, the special certification of the professional society. This is in violent contrast to 18th century England, where social mobility was great, and the person with real (or often spurious) skills or inventions could rocket to the top of cultured and learned society, much as the successful entrepreneur or entertainer rises today. Cagliostro was a Carnegie of his day.

As greater numbers clamor for and obtain acceptance in the new institution, its standards must fall. In our field of immediate interest, an increase in the level of education for the many is necessarily accompanied by a deterioration in the quality of education for the previous elite. Again, in compensation, a new level is added; and, again, it is eventually overwhelmed. Thus, the A.B. was once a mark of scholarship, then the M.A. was superposed, and later the Ph.D. Today, even this is often a "commercial degree" and various types of more advanced doctorates are in the offing—degrees comparable to the British D.Sc., which has been maintained at high standard in the Empire by allowing the Ph.D. to slide under it. Actually, such piling up of degrees and levels is an inevitable consequence of sheer size. As an army gets larger, it is necessary to create ever new top grades, of generals and super-generals, in order to satisfy the hierarchical needs of the organization. Similarly, as the base of education is widened, there must be an ever increasing number of levels up to the apex.

CONTROLLING FACTORS

It is now time to examine more particularly the various factors that determine whether a new institution will or will not form and succeed; and to point this up explicitly for institutions dispensing higher education. Some half dozen precipitating factors or conditions which must be present to an adequate extent include: a social need for the institution; a state of social stress and transition; a personal leader; sufficient funds; adequate content in the new field; and personal interest of community leaders. Conversely, the retarding factors must not be too intense, for it is usually far easier to block than to initiate something. Important retarding factors are: conservatism, whether public or institutional; vested interests; unawareness of changing goals; difficulty of reorganizing subject matter fields; and conflict between teaching and research. Once an institution is born, the consolidating factors that favor its maintenance and growth are: prestige of the individual or institutional backers; action of accreditation and supporting agencies; trustees and other officials in power. Finally, come the usual debasing factors: success itself; increasing size; progressive routinization; capture by special interest groups; and the sheer triumph of conservative routine over creative imagination. Many of these factors seem

obvious enough; in the following paragraphs I shall extend or exemplify only certain ones.

Among the *precipitating factors* is a need; the local setting must be such that the product or activities of the institution will be useful to the wider community. Thus, the rise of capitalism created the need for new utilitarian skills and subject matters, and the dominant middle class in the United States pressed for such utilitarian education. The institutionalization of science (perhaps in contrast to the content of science, as mentioned earlier) lagged in this country until after the Civil War. As Stern says (p. 153), "Although the pioneers favored utilitarian rather than literary education, scientific and technical education grew slowly in this country because the social and economic situation before the American Civil War made relatively meager technical demands upon educational institutions."

Even when need has become great, action to meet it is often delayed until a time of general social upheaval and readjustment. The saltation is especially likely to be related to a war. The Ecole Polytechnique, which revolutionized the teaching of science and introduced the modern laboratory, was created in France at the time of, and clearly in relation to, the French Revolution. Our own highly seminal land-grant act was associated with the Civil War, as was the creation of the National Academy of Sciences. Each war, in fact, has led to the creation of a major scientific institution—the Academy with the Civil War, the National Research Council with World War I, the Atomic Energy Commission with World War II; and with each war, incidentally, the attention to and the support of science by the government has made a discontinuous jump upward. I should like to note one other historical datum, although I am not sufficiently informed to guess the explanation. The first important institutions in science were the national academies, such as the Royal Society in England, all of which were struggling into a precarious existence during the first half of the 17th century. During this stage they were outcasts, even clandestine; the amorphous responses to the growing interests of a few devotees. Then, within a few years, each of these academies acquired a royal and powerful sponsor. Why Charles II chartered the Royal Society of London, Louis XIV, the French Academy of Sciences in Paris, and the Medici, the Florentine Academy of Experimentation, all between 1657 and 1666, I am not sufficient historian to surmise.

The next important factor is the personal leader, the innovator or integrator or promoter with sufficient dedication and charisma to gather a band of followers with missionary zeal. Here one could parade the familiar list of great pioneers in higher education, but I shall content myself with two instances of the aphorism, "An institution is the shadow of a man." Casual reading in the history of science and education had left me with a vague impression that the University of Michigan had played a particularly important role in American developments. The explanation apparently is that it was early blessed by having, in Tappan and Angell, two of the great educational leaders as its presidents.[2]

The other example is from Europe. In the mid 30's I visited nearly all the major universities in Britain and on the Continent, while making a survey of neurological and psychiatric research for the Rockefeller Foundation, and quickly became aware of the general decline in excellence as one moved eastward in Europe. Hungary, however, was an oasis of active research among its neighbors, and good men were busy in well built and equipped laboratories. Some inquiry suggested that this happy situation could be traced to the post World War I Hungarian government and particularly to Count Klebelsberg, then Minister of Education. The Count was a man of power, wealth, and vision and obtained unusually large funds and support for his universities.

If, indeed, a great institution is built by a great man, it is perhaps understandable that those universities controlled strongly by the faculty have not held up as well as the best of those with a strong administrative leadership. In fact, the generalization has been made that non-faculty controlled universities are commonly more progressive than faculty controlled ones. If this can be defended, some serious re-examination of our cherished beliefs is in order.

[2] I was not connected with the University of Michigan when this statement was made.

That funds constitute a major factor is obvious, and it has mattered little whether they came from private donors or from government. One need only recall numerous instances in which success came to a new or struggling institution when considerable financial support arrived. It is hardly necessary to name the benefactor of the Sheffield School of Science at Yale, the Lawrence Scientific School at Harvard, the Wharton School of Business at Pennsylvania, the Rensselaer Polytechnic Institute, the Smithsonian Institution, the Royal Academy of Science, the Rockefeller Institute, the land-grant colleges, and so on and on.

The importance of funds is also well illustrated in the timely example of the industrial institutes, now so prevalent and successful in this country. The Battelle Institute in Ohio, the Franklin Institute in Pennsylvania, the Southwest Institute in Texas, the Armour Research Institute in Illinois, and the Stanford Research Institute in California are examples of the hundreds of burgeoning and increasingly wealthy organizations occupying this niche in contemporary society. These institutions give direct service to industry, are paid well, and use their "profits" to improve facilities, enlarge staffs, and raise salaries. These institutes are already beginning to compete with universities for top level persons, and their future development will bear careful and interested attention.

The next factor is the degree of development of the field. Obviously there must exist, before a particular institute can come into being: a reasonable body of knowledge, know-how, or skills in the area; a minimal number of students interested in acquiring it; certain clienteles or customers for the output, whether this be graduates, gadgets, or gleanings. What the critical amounts may be, in content, students, or users, I do not know; but I suspect that here is another significant and important area for empirical study.

Finally, among the precipitating factors, the interest of leaders in the community or society deserves mention. If a new venture gains the support of that handful of individuals whose opinions are accepted and whose actions are imitated, it may prosper even with little initial support at a wider social base. Today we depend on the tremendous impact of the mass media to influence the public—many of us, for example, have urged such campaigns to educate the American people as to the importance of basic vis-à-vis applied research, but it is perhaps more important, and certainly incomparably more economical, to address the makers of opinion and the takers of action—the dominant legislators, businessmen, and professional people. In the early 19th century "Society" became intrigued with scientific experimentation and "salon science" flourished. To a considerable extent, especially at first, the interest was strictly dilettante and the elite sought only amusement; but a very significant number became genuinely interested in experiments. They read extensively and participated actively in the researches, and, as they became proficient in and made contributions to the science of their days, obtained both personal satisfaction and social prestige. With such interests of the leaders, there soon developed an extensive popularization of science among the educated of the day. Addison and Steele, in the *Tattler* and the *Spectator*, brought quality science to quality people in a quality manner. (I suppose the closest counterpart in this country today, if one excludes such journals as the semi-technical *Scientific American*, would be the science in *Life*, *Reader's Digest*, *New Yorker* (occasionally), and the like.) In France, also, the Academy deliberately undertook a great campaign of science popularization; and Huxley's heroic efforts a century ago undoubtedly contributed to the spectacular rise of British science.

We turn now to the less pleasant but no less important topic of the *retarding factors*. A ubiquitous one is the conservative habit of thought, with adherence to the traditional and suspicion of the novel. The institutional colleague or administrator who automatically rises to defend the status quo and is almost clairvoyant as to the perils that lurk in any change, is too familiar to require comment. Traditionalism of the public or its leaders, the spirit of a community, operates less clearly but more pervasively. It seems to be agreed that Jefferson's splendid vision for the University of Virginia failed of consummation primarily because of the

attitudes of the religious and educational leaders and especially the arrogance of leading slave-owners in the community. This so impressed William Rogers, himself a Virginian, that he sought a more favorable intellectual climate for founding a technological institute and, on this basis, located Massachusetts Institute of Technology in Boston.

Closely related, perhaps really a subhead, is the anti-intellectual attitude, especially prevalent among self-made practical men. Theory, "la'rnin'," bookishness are still suspect, and "those smart college boys" are practically subversive in the view of many. Parenthetically, I have sometimes wondered whether the scorn is simply an overcompensated inferiority. At least, the educated intellectual on the whole really is more able and effective in local affairs, civic or other, and, especially if he has entered the world of business and industry and made a success, is likely to be a power. The tribal medicine man held his high status by wit and wisdom; and I suspect that now, as then, sour opinions to the contrary are sublimations of a sour-grape mentality of the opiners.

Another basis of conservatism is religion, and the statement seems adequately supported that secular universities are likely to move more rapidly than denominational ones. In the 19th century, science was considered anti-religious, liberal, and therefore dangerous; and this attitude on the part of the public militated against the acceptance of science in universities and against universities that accepted it. In fact, the attitude may have been very much as at present!

The very college graduates constituted another conservative block. These self-recognized elite insisted that colleges give the kind of education that they had received (as alumni do today) and, therefore, favored the traditional classical education and opposed the new subjects—in this case modern science. Yet, while the university men in the east were opposing science, according to Chittenden (Stern, p. 197), the young men in the midwest were clamoring for it. In fact, while the centers of "entrenched culture" were opposing the inclusion of science in college curricula, it was being demanded in more peripheral locales by the same sort of "right people."

As mentioned earlier, there has always been an objection to hand work on the part of the elite, probably deriving from the dichotomy between free man and slave in ancient times; and the difficulty that this attitude presented to the advance of science has also been noted. The doctor made the diagnosis, the barber did the surgery; the anatomy professor described the organs, his servant dissected them and, although the exhibited structures were very different from the description, this caused no bother for centuries. It is not surprising that the laboratory method of teaching science came into being, even in the German universities, where it was so vigorously espoused, not much more than a century ago. The chemistry laboratory, the astronomy observatory, the bedside method of teaching medicine, the case method of teaching law, and the use of case materials in teaching social science are successive innovations continuing right to the present.

Another major retarding factor is the existence of inevitable vested interests. The faculties of a university, interested in their prerogatives and prestige, are reluctant to dilute these perquisites of status by sharing them with parvenus. Still more tangible, is the fear of competition for concrete goods. The existing structure, the department or the school or the university, does not want additional competitors for the, always limited, funds or space or students. When Liebig fought to introduce laboratory methods into scientific research and teaching in Germany, there was no real trouble until he tried to obtain some of the University of Berlin budget for his laboratory. The scramble for money soon became a roaring ideological argument; and unhealthy consequences of the embittered battle between bench scientists and armchair philosophers remain today—such as the suspicion most laboratory manipulators have for desk theorists.

A second example of vested interests is afforded by the failure of all attempts to modify the existing four-year college educational unit. All logical arguments seem to

favor the division of the college in the middle, the first two years, whether or not attached to a high school unit, completing general education, and the last two years initiating specialized graduate education. Such a restructuring of educational time would seem of advantage to the student, to the community, to the high schools and junior colleges, and to the graduate institutions; but Dr. Cowley is probably correct in his firm prediction that future attempts to achieve this will continue to fail. Most of the leaders in American higher education have fought for such a division and the logic of the situation favors it; but a thousand liberal arts colleges feel threatened by it and their collective power will prevent it in the finite future.

As a final instance of vested interest, I cite the departmental structure of contemporary universities. Nearly all of those concerned with higher education are aware that the existing departments no longer represent a realistic or useful compartmentalization of knowledge. In content, in methodology, in objectives, the researches of today cut across disciplinary lines. Yet the departments stand as the strongest unit structures in a university and often block any restructuring of teaching or research or, particularly, organization; interdisciplinary ventures have generally been checked by them. I suggest that the reason for the great and enduring strength of the university department is its congruence with the existing professional structure throughout the country and world. A professor of anthropology or of zoology is not only a professor in his particular university but he is a member or officer of his national society and a contributor to or editor of his specialty journal; and he prizes his status with his professional colleagues usually more than his status with his institutional ones. Only as the newer groupings establish themselves on the national scene, with their own worthy and respected organs, will there be an effective regrouping of the power structure within the universities—and by that time, of course, the regrouping will itself be outmoded and an even newer one be clamoring for existence.

In any event, another retarding factor is this very difficulty of reorganizing a field. As repeatedly pointed out, the stronger the existing structure, the harder it is to change it; and the strongest structures today are the "vertical" departments concerned with particular areas of knowledge. The current needs are for a "horizontal" structure so that the divisions are not between disciplines—psychology, physiology, and sociology; but between problems—of behavior, communication and information, system properties, and the like, and the methods appropriate to their attack and solution.

For completeness, I mention again the other retarding factors, failure to note changing goals and conflict between teaching and research; but omit any further discussion of them.

The *consolidating factors*, which favor continuity and success of a new venture, need little more than reiteration. If the man or institution behind an innovation has great prestige, as compared with little, the venture is not only more likely to succeed locally; but also is likely to be taken up elsewhere and become established on the national scene. Statements by Pusey as President of Appleton College made, at most, some local newspapers; his statements as President of Harvard are national headlines. In the same way, support for the March of Dimes skyrocketed when the infantile paralysis cause was supported by President Roosevelt. Aside from prestige, other types of support and, especially, of accreditation, have a consolidating influence. Acceptance of some new entity by a national or regional accrediting agency, its recognition by the appropriate professional organizations, interest expressed in or studies made of its area by a great foundation or other powerful body, may help establish it, quite aside from actual financial support. Indeed, any favorable attention in the mass media may suffice to put on its feet some new venture. A final consolidating factor is the continuity supplied by the faculty and officials of an institution, especially, perhaps, the trustees. A self-perpetuating board of trustees may strongly maintain its particular flavor for a long time; indeed, the way in which boards of different institutions in the same com-

munity run true, each to its own form, is sometimes dramatic.

The last group of factors, which I have called the *debasing factors*, have to do with the fixation of structure and its persistence after functional needs have changed, and with the substitution of the dead symbol or form for the live idea or process. I suppose time itself is a major factor, for the second law of thermodynamics applies to men and institutions no less than to energy; but this is universal, and interest is in the variations on it. Success and size seem to be important factors favoring vulgarization and mediocrity, and operate in the ways earlier indicated. Routinization, however efficient and necessary, militates against active thought and fixes forms, just as an habitual motor act is carried on automatically and without further attention. Closely related is the relative rise of conservative mentalities; whether due to aging of the original pioneers, to differential attraction of more staid people by the more staid situation, or to differential loss of the more adventurous staff to newly emerging adventures, need not be further pursued. In all these ways, then—success, size, routine, personnel—time favors the humdrum.

A slightly different debasing factor is the pressure of special interest groups, presumably exerted by the giving or withholding of support. There has been great concern on the part of university administrators and faculty, especially in the natural sciences, that the great flow of funds from the government (and to a lesser extent from industry, or even from foundations) would convert or subvert universities as tools to the particular and limited ends of the donor. The danger is clear and real, but whether it has in fact produced abuses is not yet certain. Perhaps the acute awareness of the danger is itself a sufficient protection against serious misuse of the potential power; but perhaps the trouble is yet to come. As Hollis (2) says (Stern, p. 353):

> Another set of social forces now shaping graduate education is associated with the growing contest between government and private enterprise for the control of research. Each part has come to see in research a national asset of greater ultimate value than the natural resources it is currently developing. The issue is clear: power over the means of scientific research and technological progress offers the surest of all ways of regulating modern business—especially big business whose monopoly or competitive status depends on trade secrets, patents, and cartels. Since the graduate school is the primary agency through which research workers are trained, it is in the nature of things of interest to both contending parties.

THE RESEARCH INSTITUTE

The particular interest of this seminar has been the research institute. Most of what has been discussed above in general is applicable in particular to an institute. Indeed, the institute is today one of the important organs still in a period of trial if not actually of birth. There are independent institutes, affiliated ones, and those which are integral parts of a university; there are institutes with industrial or technological orientation and those committed to the most abstract theoretical work; there are institutes that are the cynosure of ordinary university professors and those that respectable professors shun. It may be of some use to examine, in light of the above, what determines the value put upon a particular institute. Specifically, in a given university, what determines whether a given institute is elevated to a status above or is depreciated to one below that of a standard department?

Certainly an element of major importance is the freedom of the investigator. To the extent that he is free in choosing the problem on which he works, in discussing his ideas and observations, and in publishing his results, the institute where he works will be respected. A related criterion is the significance of the research problem. To the extent that the object of investigation is trivial in scope and narrow in interest—and I deliberately choose this criterion rather than one of applied versus basic or of practical versus theoretical, since the latter pairs of antonyms do not necessarily correspond to that of trivial or significant—the less will be the prestige of the institute and its personnel in the broader academic group. On the administrative side, similarly, if members of the institute do not have tenure or regular university appointments, and if their training and interests are peripheral to those of the men in the most closely related depart-

ments, the prestige of the institute will suffer. Finally, if the institute is staffed by men of low caliber, as judged by their colleagues in the field, and if the area of the institute is still amorphous or small—if there are not textbooks and other educational materials in this area, no journals devoted to it, no established societies of repute, in general if there is no adequate audience for its output—the institute will be in a low status position.

Last, I present in somewhat more detail the situation at Johns Hopkins and at Clark Universities, based largely upon Hollis' excellent article. Hopkins under Daniel Coit Gilman and Clark under G. Stanley Hall were carefully planned for the achievement of possible goals, on the basis of analyzed experiences with the realities of the American scene. The major policy decisions for Hopkins were made by Gilman, with the exception of two specified by Mary Garrett, who gave succor with an additional endowment when the original one by Hopkins proved insufficient. (Her conditions for support were that the school should admit women on an equal basis with men and that the bachelor's degree should be required for admission to the medical school, both of which worked beneficently.)

Gilman looked at the mishaps of other graduate schools and leaders and made plans to avoid them. At the University of Michigan, President Henry Tappan had attempted to, "adapt the University to the present wants of the community. To this end a scientific course was organized, in distinction from a classical course," as he himself put it. This was an importation of the best current German university thinking; and for his efforts, "Tappan was forced out of office amidst a flood of abuse and censure in which he was charged with trying to Europeanise the University through un-American doctrines, with 'imitating English aristocracy, German mysticism, Prussian imperiousness, and Parisian nonsensities. . . .' While it may not be complimentary to their courage, it is small wonder that the sponsors of the Johns Hopkins venture wanted to avoid conflict with these rampant political forces of frontier democracy and narrow nationalism." (Stern, p. 227)

Gilman looked at Cornell University and saw the troubles that resulted from "developing concurrently a graduate and several undergraduate schools," which tied graduate work far too closely to undergraduate needs and so throttled its own development. Gilman was also aware of the demand in Europe to "close the godless universities and depose the atheistic professors" and "knew that this demand arose because the universities produced and disseminated research on higher criticism of the Bible, the Darwinian theory of evolution, mechanistic explanations of the universe, and pragmatic philosophy and ethics. Many citizens both in Europe and the United States were convinced, or at least apprehensive, that these activities constituted threats to the existing order in religion, government, and society in general. The man in the street has always sensed lurking danger in any meddling in the folkways or any critical examination of what he thinks of as the eternal verities." (Stern, p. 229)

To avoid such conflicts and pressures, while preserving their own necessary educational goals, the Hopkins planners accordingly decided to omit a theological school in their new University. Moreover, a "good deal of sectarian opposition to the science program, which was to be the heart of the institution, was averted by centering this work on the sciences basic to the study of medicine. Public opinion trusted science in its capacity of improving the healing arts." (Stern, p. 230) Finally, after studying the great controversy in England between defenders of the classics and proponents of science, Gilman decided on science. This one thing they did "in the face of hostility from orthodox American educators and churchmen who believed that the classical studies contained the essence of truth, and who thought of the mind as a series of faculties to be disciplined." (Stern, p. 230)

Success depended not merely on avoiding dangers, but also on imaginative inventions. Graduate students were welcomed from all institutions and well-paying fellowships were made available. Research was fostered by a fluid curriculum, with the student spending much time in the laboratory to

make observations and get "elbow teaching," and in seminars where free discussion prevailed and fresh viewpoints were brought in by a flow of visiting professors. Relations between teachers and students were throughout informal and intimate. Finally, publication of significant theses was expected and a university press was created to ensure an outlet for the scholarly products.

In many respects the early history of Clark paralleled that of Hopkins, with a great leader, deliberate and thoughtful planning, a spirit of adventure but not of foolhardiness, funds, and the like. As Ryan says in his discussion of Clark University (5) (Stern, p. 333),

> Throughout the Clark University development a few rather simple elements stand forth: Designation at the start of an educational leader with a background of rich experience, powerful drive, and a broad philosophy of education; a determination to build on the basis of ascertained needs rather than traditional academic patterns; careful selection of teaching staff and students in the light of these needs, with emphasis upon "creative" learning and research of graduate quality because that area was not then occupied to any large extent by other higher institutions; an informal relationship between faculty and students, in which the cooperative possibilities of the search for truth were emphasized, culminating in a surpassingly effective use of the "seminar" in which were combined the features of coordinated planning and pooled experiences, independent exploration into a wide variety of fields of knowledge, inspired direction of the advanced work of mature students, and magnificent lecturing of an unconventional order.

CLOSE

Here, then, are some of the influences that lead to the birth and death, the success and failure, of new institutions. Universities, graduate schools, special institutes, however the structure or organ be named, are continually coming into being to meet the changing needs of a changing society and, as regularly, continue to meet these needs when they no longer exist. As Ryan well put it (Stern, p. 329),

> At Clark, as at Hopkins, the purpose was to meet what were conceived to be the needs of the day, rather than merely to follow a college and university tradition. Simple as this principle is, it is probably the most difficult to invoke consistently and the easiest to forget. It involves constantly re-facing the educational task in the light of the changes in human society. Of course there is always something a little uncomfortable about this; it presupposes a restlessness and willingness to change that become less and less characteristic of people and institutions after a time. No human enterprises are immune; the daring adventurer of yesterday becomes the stereotype of today, still talking as if he were blazing trails; and institutions are even more so.

But this is not unique to institutions of learning or even to institutions in general. Continuous change and adjustment is the price of being alive. New questions arise and nature or man spawns answers to them. The successful few of these survive and persist as individuals or species or institutions or societies. But sooner or later the questions change, the answers no longer fit, and the cycle repeats. "For," in O'Shaughnessy's fine words, "each age is an age that is dying or one that is coming to birth."

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COMMENT

The Stochastic and the "Teleological" Rationales of Certain Distributions and the So-Called Principle of Least Effort.

To what extent do the same statistical regularities found in widely different contexts indicate an operation of a "natural law" governing all the situations in which the regularities occur? G. K. Zipf thought he discovered such a law of behavior and called it the Principle of Least Effort. While his arguments are far from convincing, a number of investigators undertook to derive the regularities, pointed out by Zipf, from some general principles with mathematical rigor. Two such studies are examined and compared.

In 1949 the late G. K. Zipf published a book[1], the result of many years of painstaking labor, in which he purported to show that behavior is governed by a law of all-pervading generality, comparable to that of the Principle of Least Action in physics. Zipf called this law the Principle of Least Effort, following earlier uses of this term by other authors in certain special situations.[2] From Zipf's book it appears that he believed his contribution to be a generalization of the Principle, a discovery that it applied (in somewhat modified form) to practically all behavior if behavior is seen in sufficiently broad perspective.

An examination of Zipf's Principle fails to reveal anything resembling the Principle of Least Action in physics or, indeed, any physical law. A physical law appears as an equation whose variables are defined operationally, that is, in terms which indicate at least indirectly what one is supposed to measure *and how* to put the law to a test. By implication, therefore, a physical law indicates how it can be refuted: if certain measurements do *not* yield certain quantities, the law can be said not to hold or not to apply in the situation examined. Since no such equation describes the Principle of Least Effort, no claim can be made for it to a position comparable to a physical law. At this point a physicist or a mathematician who might have been intrigued by one of Zipf's opening statements, "every individual's movement . . . will always tend to be governed by one single principle. . ." could justifiably dismiss Zipf's claim and take no further interest in the Principle. A little reflection, however, may lead to a somewhat more tolerant examination of the underlying ideas. The term "principle" or "law" is not confined to physics with its mathematical and operational standards of rigor. Certainly the principle of natural selection in biology, homeostasis in physiology (the self-regulating action of organisms which keep the "internal environment" within limits of tolerance), and the remarkable regularities of linguistic change deserve to be subsumed at least descriptively under "principles," perhaps "laws," although in many cases no quantitative or even strictly operational description of the observations on which these principles are based can be given.

Can it be, then, that the Principle of Least Effort is to be understood in similar terms? Is it a broad generalization from observations which provides the behavioral scientist with a framework of thought (as the principle of natural selection or even the older principle of directed organic evolution has provided the biologist with a framework of thought), which can lead the behavioral scientist to formulate questions,

[1] G. K. Zipf. *Human behavior and the principle of least effort.* Cambridge, Mass.: Addison-Wesley, 1949.

[2] E.g., R. H. Waters, The principle of least effort in learning, *J. gen. Psychol.*, 1937, 16, 3–20.

design experiments, construct more precise theories, which otherwise would not have occurred to him? This question is not easy to answer. It can be answered, if at all, only in retrospect and even so not very clearly. For even if experiments and theories do appear which can be said to have been instigated by ideas immanent in what Zipf calls the Principle of Least Effort, who can trace the paternity of ideas?

Zipf himself implies that the Principle is analogous to the physical Principle of Least Action. The ideas behind the recent more sophisticated treatment of problems which had preoccupied Zipf might also have sprung directly from attempts to examine the broader implications of that principle or from questions concerning the existence of analogous principles in other areas, or, indeed, from very similar ideas in thermodynamics, mathematical genetics, and economics.

Our task is to examine the basic content of those ideas, and this could be done without any reference to Zipf's work. Yet one feels that reference should be made, because several mathematically competent investigators, who have recently brought this content to the forefront *are* familiar with Zipf's work and make repeated references to it. In a way, this is surprising, because Zipf's treatment reveals an appalling lack of mathematical understanding, to the extent of making much of his reasoning either trivial or invalid. From another point of view, however, Zipf's undertaking, because of its sheer magnitude and apparent sincerity cannot be summarily dismissed. What he did was to collect a vast number of instances of a particular type of statistical distribution (to be described below) and then try to make "sense" of his collection. The effort was prodigious and somewhat pathetic. Zipf seemed to have lacked even elementary mathematical knowledge which would have helped him at least to talk intelligently about the quantitative aspects of the data he examined. Thus, instead of applying well-known techniques of mathematical analysis and deduction to see whether the frequently recurring distributions were consequences of some reasonable assumptions, Zipf purported to see in the regularity of statistical distributions the operation of cosmic laws of behavior. These he attempted to establish by quasi-mathematical arguments, which carry no force, because, appealing to mathematical rigor, they invite mathematical standards of evaluation and so destroy themselves. Yet Zipf's work was not in vain. The attention of mathematically competent people *was* attracted to his work, and subsequent attempts to "make sense" from the data were carried out on mathematically respectable levels.

We will examine two such recent attempts. As it turns out, one of them makes shambles of the Principle of Least Effort, while the other salvages a part of the Principle. There is therefore room for two apparently different theoretical approaches to the data gathered by Zipf and by others interested in these distributions. Two different equally successful theoretical explanations to a set of phenomena should always be welcome in science. Some people will take sides, will become involved, and will put out efforts to gather evidence for their side. A competitive situation results which in many instances carries the search for truth by the momentum of enthusiasm for each side. (Cf. the development of the modern theory of combustion, spurred on by the partisanship of the adherents to the older phlogiston theory; of modern notions of reproduction of organisms, resisted by the adherents of "spontaneous generation"; contemporary controversies concerning the mechanisms of neural transmission, etc.) Also mediators often appear, willing to take into account the arguments on both sides, sometimes effecting creative syntheses, even to the extent of showing the controversies to be only of verbal nature. (Cf. the merging of the Heisenberg and the Schroedinger-de Broglie approaches to quantum mechanics through the proof of their mathematical equivalence.) Sometimes the different points of view turn out to be special cases of a more general point of view. (Cf. the heredity-vs.-environment dispute). At any rate, science (unlike some other areas of human activity) usually thrives on controversy derived from honest convictions.

The approaches to Zipf's data which we

will examine will be those of Herbert Simon and of Benoit Mandelbrot. We will call Simon's approach stochastic, a term with which no one will quarrel, and Mandelbrot's "teleological," a fighting word in some circles, which necessitates the protection of quotation marks. We will use this loaded term advisedly, however, because the attacks on it give us an opportunity to explain just how this word applies in a mathematical theory and why its connotations are useful. There is no question that Zipf's own theory of least effort is teleological in the ordinary sense of that term. By retaining the term to describe Mandelbrot's theory we seek to indicate just what it is about the teleological point of view that makes sense to the mathematician. When that sense is spelled out, the relation of the teleological to the stochastic point of view becomes clear. We now turn to Zipf's formulation of the Principle of Least Effort.

Zipf's notion of the Principle of Least Effort

Zipf notes that the typically human situation involving a goal and a set of choices of means can be schematically represented by a set of paths connecting two points. The first point represents the present situation; the second the goal to be reached; the different paths are the different means of reaching the goal. Thus, to use Zipf's example, to go from one town to another on the other side of a mountain, one can (*a*) bore through the mountain; (*b*) climb over the mountain; (*c*) go around the mountain. Each of these paths has its advantages. Boring through the mountain minimizes the distance traveled (at the cost of much time and work); climbing over the mountain may minimize the time (at the cost of much work); going around will minimize work.

The Principle of Least Effort, as stated by Zipf, requires that in the totality of situations faced by an individual (or, perhaps, a community—it is not clear whether the Principle applies primarily to one or another type of org or to all orgs), the paths will be chosen which will result in "minimizing the probable average rate of work expenditure over time." The key word "probable" enables Zipf to "explain" all situations in terms of this principle and, as we will presently see, makes the principle scientifically worthless as stated. Another serious source of difficulty is the term "work expenditure"; still another "over time." But first let us see what the operational meaning of the Principle *would* be if it had operational meaning.

Suppose we ignored the word "probable" and assumed the span of time over which the rate of work expenditure was to be averaged known; and suppose further that work expenditure were defined in the usual physical sense. The set of choices, then, which an individual (or some other org which can make choices) faces, "maps" upon a set of functions $f_i(t)$ $(0 \leqq t \leqq T)$. Each of these functions represents the course of the rate of work expenditure (i.e., power) over time. The average is obtained by integrating $f_i(t)$ over the interval $(0 \leqq t \leqq T)$ and dividing by T. If T is the same for all f_i, then to minimize the average means to minimize the total work. If, on the other hand, there is an interval T_i associated with each $f_i(t)$, then the Principle of Least Effort says that the choice of action will be so determined as to make $\int_0^T f_i(t)\, dt/T_i$ a minimum with respect to i.

In this form, the statement of the Principle is a perfectly respectable hypothesis We will mention in passing that so stated, the Principle is not identical with the well-known Principle of Least Action in physics. The units of "action" are those of energy multiplied by time (or of momentum multiplied by distance). If we translate Zipf's terms into their physical meanings, then Zipf minimizes average power, that is work *divided* by time or else action divided by the square of time. While the physical Principle of Least Action is a logical consequence of Newtonian mechanics, we cannot think of any law of physics from which the Principle of Least Effort could be derived. We suppose, therefore, that the Principle of Least Effort is an independently (empirically?) discovered principle applicable to behavior of organisms. When we inquire, however, into its possible meaning in terms of behavior, particularly of the kinds of behavior studied by Zipf, we fail to

see any meaning. For one thing, what is the "rate of expenditure of work" in the behavioral sense? Are we to attribute to it the usual physical meaning (scalar product of force and velocity vectors)? Certainly it is far fetched to see a connection between such a quantity and the "work" of "searching for words," as in human communication or the "work" involved in the "conflict within the super-ego" which Zipf endeavors to subsume under his Principle. And even if such translations could be made (functions analogous to "work expenditure" could be found), Zipf's definition of "probable" destroys the last skimpy claim of the Principle of Least Effort to any operational meaning. For Zipf implies that the functions $f_i(t)$ as well as the interval T_i are *estimates of the actor*. As subjective estimates, they are not ascertainable from reports, because few people and no animals are able or willing to state in quantitative terms how much work expenditure they expect will be associated with each course of action (remember the estimate has to be made over the entire remaining life span and possibly over the life span of future generations!) Occasionally a vague, rough estimate of sorts may be given, but certainly no minimization principle can be deduced from such data, for one does not even know to what extent such data are reproducible. There remains the recourse of deducing the actor's estimates from his choices. But this makes the "theory" entirely circular, i.e., designed to explain the choices by the supposed estimates and the supposed estimates by the choices. In other words, the theory provides no opportunity for its own refutation and ceases to be a theory.

In fairness it must be pointed out that the seeming circularity of some theories does not automatically make them worthless. Recent investigations aimed at estimating "subjective probabilities," that is, degrees of belief attributed by subjects to forthcoming events come up against the difficulty of disentangling the parameters pertaining to these beliefs from those pertaining to the utilities attributed by the subject to various possible outcomes. Typically attempts are made to estimate beliefs from the subject's "betting strategies," which, however, involve these utilities. Thus the subjective probabilities cannot be determined without knowledge of the utilities, and vice versa. This circularity does not make the investigations worthless provided they have adhered to certain standards of rigor as far as they have gone. Even though no final answers can be obtained until the probabilities and the utilities have been separately determined, yet the theory has been brought to a certain point yielding clear indications of the problems still to be solved. Unfortunately no such analysis is evident in Zipf's treatment of the Principle of Least Effort. His arguments are vague appeals to the recognition of the Principle in a great variety of situations simply on the basis of its plausibility. And even these appeals are often stretched far beyond ordinary credibility. Thus Zipf considers as evidence of the Principle of Least Effort the so-called "vocabulary balance" based on "the economy of words":

> From the viewpoint of the speaker (the *speaker's economy*) who has the job of selecting not only the meanings to be conveyed but also the words that will convey them, there would doubtless exist an important latent economy in a vocabulary that consisted exclusively of one single word—a single word that would mean whatever the speaker wanted it to mean. Thus if there were m different meanings to be verbalized, this word would have m different meanings. For by having a single-word vocabulary the speaker would be spared the effort that is necessary to acquire and maintain a large vocabulary and to select particular words with particular meanings from this vocabulary. The single-word vocabulary, which reflects the *speaker's economy*, may be likened to an imaginary carpentry kit that consists of a single tool of such art that it can be used exclusively for all the m different tasks of sawing, hammering, drilling, and the like, thereby saving the labor of otherwise devising, maintaining, and using a more elaborate toolage.
>
> But from the point of view of the auditor (the *auditor's economy*) a single-word vocabulary would represent the acme of verbal labor, since he would be faced by the impossible task of determining the particular meaning to which the single word in a given situation might refer. Indeed from the viewpoint of the auditor, who has the job of deciphering the speaker's meanings, the important internal economy of speech would be found rather in a vocabulary of such size that it possessed a distinctly different word for each different mean-

ing to be verbalized. Thus if there were m different meanings, there would be m different words, with one meaning per word. This one-to-one correspondence between different words and different meanings, which represents the auditor's economy, would save effort for the auditor in his attempt to determine the particular meaning to which a given spoken word referred.[3]

Having thus established two opposing "forces" which supposedly determine the evolution of vocabulary, Zipf views the actual distribution of meanings among words as the result of their balance. And since the "forces" are supposed to be instances of the Principle of Least Effort (though one is at a loss to guess how these "forces" can be deduced from the Principle as originally stated), so is the balance.

Turning to empirical evidence of "vocabulary balance," Zipf cites the now well-known approximately hyperbolic relation between frequency rank order and frequency of occurrence of words in large samples of language. This has nothing to do with meaning distributions, but a great deal is made of the fact that rank order times frequency is nearly constant (which is simply what a hyperbolic distribution means), and moreover that this approximate constant multiplied by 10 gives the total approximate word length of the sample examined (James Joyce's *Ulysses*). Again one is at a loss to see how it could be otherwise, since no special significance is assigned to the factor 10. Is not the product of one constant with another *bound* to give any desired constant, provided one of the factors is suitably chosen?

The distribution of meanings over words (the most frequent having, of course, the greatest number of meanings) is also introduced as evidence. But again, aside from the obvious conclusion that the distribution of meanings falls between the extreme ideals of one-word-all-meanings (the "speaker's economy") and one-word-one-meaning (the auditor's economy) not much can be seen to corroborate the almost mathematically stated Principle of Least Effort.

These instances of extreme gaucherie are mentioned here not with a view of criticizing Zipf's work at this late date (this job was done by responsible critics several years ago) but on the contrary to show how even most unskilled efforts of theory construction, verging on total incompetence, may carry in them fruitful germs of future theories. The fact, as we have said, is that Zipf's extremely raw ideas did give an impetus to theoretical constructions of considerable interest. The Principle of Least Effort in its grandiose form fell by the wayside, but it took some serious mathematical thinking to show that the distributions which so intrigued Zipf could be deduced without any appeal to such a principle (Simon's stochastic approach) and some other serious mathematical thinking to show how if the principle were more modestly conceived and more precisely stated, it could be preserved as a possibly useful theoretical tool in research on the statistical structure of language (Mandelbrot's "teleological" approach).

The stochastic and the "teleological" derivations of distributions

It is now time to define the terms "stochastic" and "teleological," particularly the latter, since we use it here in a rather restricted sense. Both terms refer to particular theoretical orientations to observed regularities. The stochastic method seeks to explain regularities observed on a gross level by deducing these regularities from probabilities of a very large number of "small" events (that is, events whose individual effects on the process are very small). The "teleological" method seeks to deduce the regularity from some gross principle operating in nature so that the regularity appears as a result of directed *trends*. Statistical mechanics and classical thermodynamics furnish typical examples of the two kinds of orientation directed at the study of the same phenomena. The observed regularities in closed physical and chemical systems are trends toward equilibrium states. Statistical mechanics explains these trends by analyzing stochastic processes consisting of probabilities of minute events on the molecular level and resulting in the passage of the system from less probable to the most probable state. Phenomenological (classi-

[3] G. K. Zipf, *op. cit.*, p. 20–21.

cal) thermodynamics, on the other hand, is "teleological" (in our restricted sense). It explains the trend toward equilibrium of closed systems by postulating that a certain quantity associated with the system (entropy) can never spontaneously decrease but only increase. Equilibrium is thus calculated as the state of greatest entropy. It is as if the system constantly *sought* that state; hence the appropriateness of "teleological" as a term describing the method which puts such trends at the center of attention.

The stochastic method, then, proceeds from minute "local" events to the deduction of the gross events. "Teleological" methods seek to explain events by the operation of gross principles in which certain end states seem to be "sought," typically the attainment of maxima or minima by certain important variables. The taking of "local" events as fundamental is not confined to the stochastic methods. Classical mechanics, for example, (to the extent that it describes physical laws by differential equations) focuses on "local" events. The Principle of Least Action in physics, on the other hand, and also the relativistic observation that the path of a planet is a geodesic in space-time are "teleological." Lamarckian evolution was teleological; theories of evolution based on natural selection principles are not; modern mathematical genetics is stochastic. There have been comparatively recent attempts to construct a theory of evolution on "teleological" principles, for example to see the over-all trend as a tendency to maximize the energy flux through the entire biota.[4] "Teleological" (holistic) and non-teleological ("local" or stochastic) points of view have alternated in the history of science. They seem to complement each other as strategies and warrant no judgment about their relative merit or level of sophistication. Especially instructive are the instances when the two points of view are shown to be strictly equivalent (as for example, the Newtonian and the Hamiltonian mechanics).

[4] See, for example, A. J. Lotka, "Contribution to the energetics of evolution," *Proceedings of the National Academy of Sciences*, 1922, 8, 147.

In view of this classification of the strategies of theory construction, we see that Zipf's approach to the regularities observed in many statistical distributions was a strongly teleological one. It also happened to be devoid of theoretical rigor. We now have before us two examples of theoretically rigorous approaches to the same set of data, the non-teleological, stochastic approach of Simon and a "teleological" approach of Mandelbrot. The latter, in contrast to Zipf's attempt, attains scientific rigor by spelling out in mathematically precise terms just what it is that tends to be maximized in the evolution of a vocabulary.

Simon's stochastic derivation[5]

Simon's problem is to derive the statistical distribution in question, such as number of different words plotted against their frequency of occurrence, as a *steady state* resulting from a particular stochastic process. As an example of an elementary problem of this sort, consider a million rooms, each containing a million pennies strewn on the floor. The pennies are constantly and randomly flopping over. At a given moment each room shows a certain number of "heads" among its pennies. If we plot the number of rooms in which the number of heads occurs in a certain range (say from 435,600 to 435,700) against the corresponding range, we will obtain the well-known "normal curve" with a maximum around 500,000. Indeed, no matter what the initial distribution of heads and tails had been, after the process of flopping over at random has gone on long enough, the normal distribution will become established. The normal distribution is a "steady state" distribution, which, once established will tend to persist. Now the normal distribution is observed in a great many situations. The scores on IQ tests, weights of human beings and of beans of a given variety, horizontal and vertical distances of hits from the bull's eye in target practice, all are more or less exactly representable by normal distributions. Suppose one noticed this fact and pondered on the existence of a

[5] H. A. Simon. On a class of skew distribution functions. *Biometrika*, 1955, 42, 425–440.

"general principle" governing the growth of men and beans as well as the shooting at targets. One could conceivably come up with a "theory" to rival Zipf's. That such a "theory" is not likely to be taken seriously today is a consequence of the existence of a much less pretentious stochastic explanation of the normal distribution. Whenever a large number of quantities suffer random increments or decrements *independent of the size of the quantities* the steady state distribution of the quantities will be normal. This explanation is not equivalent to a discovery of a "natural law," but is not altogether devoid of content. Whenever we see a normal distribution, we can suppose the operation of a great many independent factors which contribute to the quantities considered by operating independently on the quantities. Thus a weak but important conjecture is that the relative size of a member of a particular species is probably determined (to the extent that it is genetically determined) by summated actions of many independent genes.

The fundamental question of the stochastic approach, then, is the following: given a steady state distribution, what stochastic process gives rise to it? In general there will be many stochastic processes which will give rise to the same steady state. Therefore if one is pursuing a theory with content (not simply a mathematical investigation), one is faced with the problem of selecting from among the possible stochastic processes the one which can be most plausibly translated into assumptions related to the phenomenon studied.

Such is the approach taken by Herbert Simon to the derivation of the distributions studied by Zipf and other distributions of similar types. Zipf has examined a great many, ranging from language samples to corporation assets. Most frequently recurring variables in the language samples are frequency of occurrence of words plotted against the rank order of frequency of occurrence. Another pair of variables is the number of *different* words of a given frequency of occurrence plotted against the frequency. It should be pointed out that these two distributions are not independent. The first is an integral of the second, for clearly "the number of words whose frequency of occurrence is greater than a given number" is equivalent to the frequency rank order of the word which has the given number for its rank. Simon pursues the analysis of this distribution in its differential, not integral form.

He notes first of all that all these distributions have the following characteristics in common. They are J-shaped with very long tails. That is to say, if the number of different words occurring with a given frequency of occurrence is plotted against the frequency, the least frequently occurring words are the most numerous. The number of words that occur only once in a sample is greater than the number of words that occur twice, which is greater than the number that occur three times, etc. It is also true that the number of authors that are mentioned once in, say, 20 years of *Chemical Abstracts* is greater than the number that are mentioned twice, etc. The "very long tails" reflect the circumstance that the falling off of number with increased frequency of occurrence becomes very gradual for large frequencies.

If i is the frequency of occurrence, then a good fit for the *tails* of these distributions (that is for large values of i) is given by the formula

$$f(i) = (a/i^k)b^i. \qquad (1)$$

The constant b is usually taken to be very nearly 1, so that unless i is very large, the presence of that factor makes no difference. If i is very large, however, b^i departs considerably from 1, being greater or less depending on whether b is greater or less than 1. In this way, a proper choice of b may make $f(i)$ approach zero faster or slower than the expression (a/i^k) to give better fits to empirical data if necessary.

The function $f(i)$ is an empirical function. It is used simply as a mathematical description of the distribution curves observed in various situations. Thus the application of this formula for a variety of distributions with widely different content indicates nothing whatsoever about the possible similarity of underlying mechanisms which give rise to the distributions. All we know at this stage is that the distributions "have

the same shape," to wit the larger the i (whatever it is) the smaller the f, and the smaller the rate of decrease of f relative to i. This is much too vague a description to be called even a properly quantitative one. Nor is the fact that the "same" mathematical formula "fits" in a variety of situations very interesting. The formula has three free parameters and so represents a rich variety of curves. It is not surprising that many sets of data can be fitted with it.

There are, however, two other restrictions on the formula, as it fits the distributions studied. To begin with, in the formulas used to fit word frequencies, publications, and urban populations, k is always greater than 1, and usually very nearly 2. The distributions dealing with numbers of authors publishing a given number of papers in a prescribed period, and also those describing the number of biological genera containing a given number of species and numbers of words occurring with a certain frequency are fitted well not only in their tails but also at their "heads." Thus the ratio $f(2)/f(1)$ is usually nearly $\frac{1}{3}$, while $f(1)/n$ (where $n = \sum_{i=1}^{\infty} f(i)$) is usually close to $\frac{1}{2}$. In the case of authors, this would mean that the number of authors publishing two papers is about $\frac{1}{3}$ of the number publishing one paper, and that the number of authors publishing just one paper is about half the total number of authors publishing during a given period.

Having described the salient characteristics of the distributions, Simon first of all suggests a formula different from (1) to describe them, namely the following:

$$f(i) = AB(i, \rho + 1). \qquad (2)$$

Here A is a constant and B is the so-called "beta function." The beta function can be best explained to the non-mathematical reader by first translating it into the "gamma function." Thus by definition

$$B(i, \rho + 1) = \frac{\Gamma(i)\Gamma(\rho + 1)}{\Gamma(i + \rho + 1)}. \qquad (3)$$

Now the gamma function is easy to describe. For any number n, $\Gamma(n) = 1 \cdot 2 \cdot 3 \cdots (n - 1)$ or, equivalently, $\Gamma(n + 1) = n!$ (n-factorial). If now a smooth curve is drawn through the values of $\Gamma(n)$ plotted for whole numbers, one obtains approximately the graph of $\Gamma(x)$ for all real $x \geqq 2$.[6]

The mathematical properties of the gamma function have been thoroughly investigated. In particular, it is known that for large values of i, the expression $\Gamma(i)/\Gamma(i + k)$ is very well approximated by i^{-k}. It appears therefore that the expression (3) is very well approximated by $i^{-(\rho+1)}$ for large values of i.

Now $\Gamma(\rho + 1)$ is a constant (independent of i). In formula (2), this constant will combine with A to form another constant. Since A is arbitrary, so will the new constant be and so need not enter our considerations. The "business" part of the formula is $\Gamma(i)/\Gamma(i + \rho + 1)$, which, for large values of i, is very well approximated by $i^{-(\rho+1)}$. Thus if $\rho + 1 = k$, we have the desired power expression i^{-k}. Moreover, if $k = 2$ (i.e., $\rho = 1$), as is the case in many situations, we have $\Gamma(i)/\Gamma(i + k) = f^*(i) = 1/i(i + 1)$ and $\sum_{i=1}^{\infty} f^*(i) = 1$, so that $f^*(2) = \frac{1}{6}$, $f^*(1) = \frac{1}{2}$, $f^*(2)/f^*(1) = f(2)/f(1) = \frac{1}{3}$ and $f(1)/n = \frac{1}{2}$, as is also typically the case in those distributions.

Thus the function proposed by Simon has all the desired characteristics. It "fits." However, as such it is only one of innumerable functions that "fit." Its great advantage over other formulas is that it not only fits but can be derived from reasonable assumptions about the underlying process that supposedly gave rise to the distribution. It is thus not an empirical but a derived formula and attains a degree of theoretical significance, which we will examine further below. Let us now examine the stochastic process.

To fix ideas (that is, to keep in mind more or less concrete referents for the mathematical abstractions) Simon examines the process underlying the accumulation of some large verbal output, say the words comprising a book or a collection of newspaper issues. Suppose we have come to the kth word in this collection and ask what

[6] Actually $\Gamma(x)$ is defined for $x > 0$, but its behavior between 0 and 2 cannot be meaningfully compared with the behavior of factorials and need not concern us here.

chances there are for the $(k + 1)$st word to be in a certain class. These classes may be defined in a variety of ways, for example by parts of speech or by the letters they contain. The particular classification which interests us is frequency of occurrence. Therefore let us divide all the words in this collection of k words into classes, designated by the index i. A word of the ith class is a word which has appeared exactly i times among the k words in our collection. We now ask how many *different* words are in the ith class. This, of course, depends on k as well as on i. We still don't know what that dependence is; so we designate the number of different words by the yet unknown function $f(i, k)$. It is clear that the object of the mathematical analysis will be to determine this function $f(i, k)$. The theoretical significance of formula (2) will have been established (to a degree) if this function $f(i, k)$ turns out to be precisely the formula (2). The analysis, however, cannot be started without some assumptions about the properties of the function $f(i, k)$, and it is these assumptions which constitute Simon's stochastic model.

There are two such assumptions, both reasonable. One states that the greater the total occurrences of the words in class i among the k words in the sample, the greater the likelihood that the $(k + 1)$st word will be in class i. Now since by definition a word in class i has occurred i times and there are $f(i, k)$ different words in that class, it follows that the total number of occurrences in that class has been $if(i, k)$. The first assumption then states that the probability that the $(k + 1)$st word will be in class i will be equal to $K(k)if(i, k)$, where $K(k)$ is the proportionality factor, independent of i but dependent on k. Indeed the dependence of K on k can be calculated and will be given below.

The other assumption states that there is a constant probability that the $(k + 1)$st word will be a new word. That such a probability exists cannot be doubted; that it is constant is open to question. For example, if the total vocabulary of the author is limited, there will come a time when he has no new words to add, so that eventually, when k is very large, the probability of a new word should be zero. Also in the beginning of the process (when k is small) the probability of words appearing for the first time (especially if the vocabulary is large) should be large. It is reasonable to suppose, therefore, that the probability of a new word keeps decreasing with k. This alternative assumption (as well as one in which the vocabulary is suddenly exhausted at some point) is introduced later as a refinement. For the time being, however, Simon supposes that there is a range of k where the probability of a new word (denoted by α) is constant. He can now write down the equations which reflect his assumptions and definitions. These are

$$\begin{aligned} f(i, k + 1) - f(i, k) &= K(k) \\ &\quad \{(i - 1)f(i - 1, k) - if(i, k)\} \\ f(1, k + 1) - f(1, k) &= \alpha - K(k)f(1, k) \\ &\qquad (i = 2 \ldots k + 1) \end{aligned} \tag{4}$$

Here the f's are no longer the actual numbers of different words in class i but the expected values of these numbers. Since they need not be whole numbers any longer, they can be considered as continuous functions of continuous variables. The equations can be seen to be the consequences of the assumptions. We will confine ourselves to showing this for the second equation. Consider the expected number of words in class 1 (that is, those that have occurred only once). If the $(k + 1)$st word is among them, the f of that class $f(1, k + 1)$ will be reduced by 1. If the $(k + 1)$st word is a new word, that class will be increased by 1. If the $(k + 1)$st word is in some other class $f(1, k + 1)$ will remain unchanged, that is, equal to $f(1, k)$. Hence the expected excess of $f(1, k + 1)$ over $f(1, k)$ is the expected excess of α (probability of increase) over $K(k)f(1, k)$ (which by the first assumption is the probability that the $(k + 1)$st word will be in class 1, that is, the probability of decrease), and this is what the second equation of (4) says.

Further considerations, involving the restriction that the exhaustive sum of probabilities must equal 1, lead to the conclusion that the proportionality constant $K(k)$ must equal $(1 - \alpha)/k$.

The difference equations (4) can be solved by well-known methods. The solution turns out to be indeed the proposed formula (2).

Simon goes on to show that this formula can be derived also in another way, namely by supposing a sample of k words to "move along" in time, catching new members at the "head end" and dropping old ones at the tail end, where again the most frequent words have also the greatest probabilities of being added or dropped. Simon then gives his rationale for supposing that stochastic models such as these may underlie the vocabulary distribution in samples of verbal output. An author chooses his words partly by association (tending to repeat words which he has already used) and partly by imitation (tending to select words commonly used). Obviously association alone cannot account for the process, because no new words could enter the sample this way. Indeed, in this case, after the first word had been somehow chosen, we would have a perfect demonstration of Zipf's "economy of the speaker"—the repetition of a single word! Nor can imitation alone be responsible, since by this principle the frequency distributions of single words would be about the same in all samples, which is, of course, far from being the case. (To use Simon's example, the proper noun "Bloom" ranks high in frequency in *Ulysses* and not in other samples, because Bloom is a character in *Ulysses*. If this hero's name were Smith, "Smith" would have the frequency rank which "Bloom" actually enjoys).

Simon then shows that both of these processes, association and imitation, are adequately reflected in his model. Thus his model does to some extent contain a behavioral theory, albeit a weak one, that is, one which presupposes little more than a minimum of constraints on pure chance, constraints consistent with generally observed behavioral patterns.

According to Simon, in the "weakness" of the model lies its strength. For then the applicability of the same steady state distributions to a wide variety of phenomena ceases to be mysterious. (Cf. our previous examples of the normal distribution.) Besides the data on word frequencies, Simon cites number of authors plotted against the number of papers they have published, data taken from several scientific journals over 10–25 years; number of cities in a given population interval plotted against the population; number of people in a given income bracket plotted against income; and number of biological genera comprising a given number of species plotted against the number of species. All of these sets of data are well fitted by the type of distribution described. Rejecting the operation of a "teleological" principle in these extremely diverse instances, Simon leans toward regarding them as instances of a certain stochastic process roughly described as follows: the larger the magnitude of a random variable, the larger the fluctuations it is likely to suffer. Thus this process differs essentially from the one which underlies the normal distribution, in which the fluctuations suffered by a random variable are independent of the magnitude it happens to possess. But except for this quantitative difference, the skew distributions noticed by Zipf and analyzed by Simon (and still earlier by Yule[7] whose results have been generalized by Simon) are the results of the same *kind* of stochastic process, fluctuations suffered by a random variable which lead to a steady state distribution. The meaning of the variable and the content of the fluctuations being entirely irrelevant to the quantitative aspects of the phenomena, it is argued in effect by Simon that it is unreasonable to suppose the operation of an underlying "teleological" principle, especially since the results can be derived from assumptions which do not include such a principle.

Mandelbrot's "teleological" approach[8]

Let us, nevertheless, examine the "teleological" principle posited by Mandelbrot.

[7] G. U. Yule. A mathematical theory of evolution, based on the conclusions of Dr. J. C. Willis, F. R. S. *Phil. Trans.* B, 1924, 213, 21; *The Statistical Study of Literary Vocabulary*, Cambridge: Cambridge University Press, 1944.

[8] Among the several accounts of this approach, the most informative is probably B. Mandelbrot, "An informational theory of statistical structure of language" in *Communication Theory* (Willis

The publication of Mandelbrot's papers dealing with this aspect of language structure antedates Simon's and possibly instigated the latter to undertake his "counter-analysis." Thus, logically a reviewer would first present Mandelbrot's theory and then Simon's alternative analysis which could be taken to challenge the *raison d'être* of the theory. We are reversing the order of presentation in order to minimize the impression that Simon's analysis necessarily refutes Mandelbrot's. As stated earlier, we think both are of great theoretical importance. We are ready to confess to more. Certain esthetic considerations are unavoidable in theoretical discussions, and so one cannot avoid wishing that there were more to the determination of the statistical structure of languages than the interplay of probabilities. Mandelbrot's theory nourishes this hope. Let there be no misunderstanding. Ours is not a nostalgic yearning for old-fashioned teleology. It is not the philosophical content of teleological constructions (with their implications of guiding agencies) which appeals to us, but the opportunity they provide for formulating general unifying principles. These two motivations for attempting teleological constructions are not identical. The theory of natural selection and later modern genetics exploded the guiding principle in evolution (to which some philosophers still cling), but it left the principle of *adaptation* intact. In short, we usually have to abandon "providential" teleology as scientific analysis progresses. But "quasi-teleological" principles are sometimes salvaged and continue to play an important part in scientific conceptualization. A "quasi-teleological" principle is one which states some law in the form of a *trend*, usually toward a maximum or a minimum of some quantity. The importance of such principles is that they single out for scrutiny quantities which form pivotal points of a theory. For example the kinetic theory of matter has been entirely reduced to statistics, but the gross trend of closed systems toward maximum entropy, although a consequence of the statistics, is still a "physical law" of prime importance serving to give unified meaning to most diverse physical and chemical phenomena.

This is why it is a pardonable bias to keep looking for such "laws," even though they may in the last analysis be shown to be only gross consequences of stochastic processes.

Mandelbrot postulated such a "law." It has a teleological flavor but it should be clear by now that we call any law which is based on a maximization or a minimization of some quantity, singled out for scrutiny, a "teleological" one. Zipf, of course, tried to do the same, but never succeeded in doing what Mandelbrot did—define the quantity precisely, state the law in mathematical form and deduce the results he was trying to explain by rigorous instead of intuitive arguments.

We turn to Mandelbrot's treatment. In the sample of language to be examined the words which occur can be ordered according to rank of occurrence. Let the most frequently occurring word be called W_1, the next most frequently occurring, W_2, etc., to W_R, the least frequently occurring word in the vocabulary of R words of the sample. We note that some words will have equal numbers of occurrences, in which case their ranks are to be assigned at random within the class determined by a given number of occurrences.

Associated with each word will be a certain frequency of occurrence, or, if we divide the frequency by the total number of words in the sample, the probability of occurrence. Call the probability of occurrence of W_r (the word of rank r) p_r.

Associated with each word there will also be a "cost" of producing it, be it the time of speaking it, or the energy, and also

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Jackson, ed.), London: Butterworths, 1953; New York: Academic Press, 1953. Those who would rather read French than mathematics will find a version for linguisticians in the same author's "Structure formelle des textes et communication: deux etudes," *Word*, 10, 1–27, 1954. Brief and clear summaries will be found in G. A. Miller, "Communication," *Annual Review of Psychology*, 5, 401–420, 1954 and in R. D. Luce, "A survey of the theory of selective information and some of its behavioral applications," *Technical Report No. 8*, Bureau of Applied Social Research, Columbia University, 1954.

the "cost" of understanding it. What this cost may depend on need not concern us now, but this question will have to be raised later. Let us at this point ask the following question. If the assignment of frequencies (or probabilities) to the several words of the vocabulary is under the control of whoever will use the language, what assignment will minimize the average cost per word? The answer is obvious and also absurd. To minimize the cost, select the "cheapest" word and assign to it probability 1, that is, speak only the cheapest word!

Here Mandelbrot comes very close to Zipf's ideas inasmuch as the notion of cost and its minimization (Zipf's "effort") enter theoretical considerations of language formation. Mandelbrot's notions depart from Zipf's in that the resulting "balance" does not come from diametrically opposed interests of speaker and listener but from something entirely different.

The trouble with the one-word language is that it fails to convey information. Thus both the speaker and the auditor suffer if their purpose is to communicate. Of course, Zipf's theorizing may still be salvaged by pointing out that he too sought a formula as a compromise between effective and economical communication. The crucial thing that is lacking in Zipf's formulation, however, is a *measure* of information and of the cost. For want of a measure all the arguments about minimization or maximization become meaningless.

To Mandelbrot, on the other hand, such a measure was available, namely Shannon's famous formula

$$H = -\sum_{i=1}^{R} p_i \log p_i , \qquad (5)$$

where the p_i is the probability of the ith "signal" in a repertoire of R signals and H is the amount of information conveyed per signal.

This formula and a formula for cost (to be discussed below) enable Mandelbrot to cast the minimization (or maximization) problem into mathematical form.

To be sure, the use of Shannon's formula in the situation examined is open to serious criticism. It has been argued that the information-theoretical treatment of communication theory cannot be simply transplanted into a "communication theory" as it is understood by linguists, psychologists, or semanticists, since the requirements of the information-theoretical model cannot be unambiguously established in the universe of meaning. However, a *caveat* need not be interpreted as a prohibition. It is certainly legitimate to *try* to apply information-theoretical ideas to the study of the statistical structure of languages if only to see what can be done with them.

And so Mandelbrot assumes that the amount of information *per word* in a sample of verbal output is given by formula (5) where the p_i are the probabilities of occurrence of the words. Clearly if one of the p_i is unity (and therefore all the others are zero), the amount of information conveyed is zero, as is intuitively evident and as the formula implies. Hence the exclusive use of the "cheapest" word trivially defeats the purpose of communication.

Mandelbrot now asks the following questions:

Given a *fixed average cost* per word, what shall be the frequency distribution of the words to give *maximum* information per word?

Or alternatively,

Given a *fixed amount of information* per word, what shall be the frequency distribution to result in the *minimum* average cost per word?

Hence his "compromise" is not one which the speaker makes with the hearer but rather one which the language user in whatever capacity makes between effectiveness of communication and its "cost."

Either of these questions maps on a clear mathematical problem. The solutions of both problems are formally identical. Let us, therefore, solve the first one. Let C_r be the "cost" of the rth word. Then the average cost per word is

$$C = \sum_{r=1}^{R} p_r C_r , \qquad (6)$$

which is to be kept constant. This is one constraint on the problem. Another constraint is

$$\sum_{r=1}^{R} p_r = 1, \tag{7}$$

which must hold because the p_r's are probabilities of mutually exclusive and exhaustive events. The problem, then, is to maximize $H = \sum_{r=1}^{R} p_r \log p_r$ subject to the constraints (6) and (7). A straightforward solution is obtained by the use of the so-called Lagrange multipliers. Since H to be maximized must be "stationary" as any of the p_r varies, and so must $\sum p_r C_r$ and $\sum p_r$, we have the equations

$$\frac{\partial}{\partial p_r}[-\sum p_r \log p_r + \lambda \sum p_r C_r + \pi \sum p_r] = 0, \quad (r = 1, 2 \cdots R) \tag{8}$$

where λ and π are multipliers, arbitrary for the time being, but to be determined from the conditions of the problem.

The system (8), solved for each p_r, gives

$$p_r = M^{-1+\pi+\lambda C_r} \tag{9}$$

where M is the base to which the logarithms are taken. The constraints on the problem then determine the values of π and λ. We note also that λ must be negative, because by the way rank was defined, p_r must be decreasing with r. If we set $M^{\pi-1} = A$ and $\lambda = -B$, we can write

$$p_r = AM^{-BC_r}. \tag{10}$$

Zipf's formula (somewhat generalized) can now be obtained from (10) if it can be shown that the "cost" C_r is a logarithmic function of rank r. This derivation is the crucial step of Mandelbrot's argument.

Indeed he derives the logarithmic relation from the way the words are "coded," presumably "optimally," that is, by minimizing the total cost. We will first give a simplified argument and then generalize it.

Suppose we are constructing a language, that is, a code in which every word will be a certain sequence of letters. Suppose there is the same cost attached to the use of each letter. We shall then want to use up all the possible one-letter words before starting to use the two letter words, next use up all the two letter words, etc., in order to minimize cost. If G letters are at our disposal, and each letter costs one "unit," there will be G words costing one unit each (i.e., of rank one, since these should be assigned the greatest frequency of occurrence), G^2 words of two units each, G^k words of k units each, etc. It follows that rank (G^k) increases exponentially as cost (k) increases linearly, or, what is the same thing, cost increases logarithmically with rank.

However, the argument does not stand or fall with the assumption that the cost of the word is proportional to the number of actual letters in it (as we write them) nor with the assumption that all letters cost the same. Mandelbrot's "letters" must be taken to be as hypothetical elements (perhaps phonemes, perhaps something else) of which words are composed. The cost of a word is the sum of the costs of the "letters," but these need not be equal. The more general equation for the number of words of a given cost C is then

$$N(C) = N(C - C_1) + N(C - C_2) + \ldots N(C - C_G) \tag{11}$$

where $C_1, C_2, \ldots C_G$ are the costs of the individual letters.[9] The solution of this equation gives to a first approximation (for large C) the relation between cost and rank

$$C_r \sim \log_M r \tag{12}$$

where M is the largest root of the equation

$$\sum_{g=1}^{G} M^{-C_g} = 1. \tag{13}$$

(This is the same M which for convenience is taken as the base of the logarithms in Shannon's formula).

As the next approximation Mandelbrot gets instead of (12),

$$C_r = \log_M (r + m) + C_0 \tag{14}$$

where m and C_0 are certain constants. If now (12) is substituted into (10), we get

[9] Cf. an identical equation governing the number of sequences of a given length in C. E. Shannon and W. Weaver, *A Mathematical Theory of Communication*, Urbana: University of Illinois Press, 1949.

$$p_r = Kr^{-B} \tag{15}$$

which is identical with Zipf's empirical formula if $B = 1$. If, however we substitute the more exact formula (14), we get

$$p_r = P(r + m)^{-B} \tag{16}$$

where P is another constant. It is noteworthy that while formula (16) fits the rank-frequency curves better than Zipf's formula $p_r = Pr^{-1}$, the correction was not imposed to make the fit better but is a consequence of a more exact solution of the underlying theoretical equation.

Mandelbrot poses two other problems. In one, he allows both R (the size of vocabulary) and C (the average cost) to vary and minimizes the average cost *per unit of information*. Here the constant cost of the "space" between the words enters the calculations and indeed is the determinant of the parameter B. In another problem, the so-called "secrecy problem," he undertakes to calculate the most economical code which is also unbreakable (i.e., can be deciphered only by trying all possible keys). In all cases the frequency-cost relation is the same. The only difference in these problems is in the way the parameters are determined.

Mandelbrot thus deduces the observed distributions by what we call a "teleological" formulation, first by minimizing the cost under certain restraints and thus obtaining the relation between frequency and cost; then by minimizing total cost by proper coding and obtaining the relation between rank order and cost.[10] The combination of the two gives the de red result, even an improvement on it.

We have already alluded to criticisms leveled against the theory. They, of course, should not be minimized. In addition to those discussed above, some other important weak spots should be mentioned. What are the "letters"? The optimum coding requires that all combinations of "letters" be admissible. No known linguistic units have this property.

The approximations to the exact solution of (11) are valid for large C, i.e., for words of many letters. How many are "many"? This question cannot be answered without some knowledge about the character of the "letters."

Mandelbrot does cite some evidence to indicate that there may be a "cost" associated with certain hypothetical linguistic units. It seems that the time it takes to recognize a word varies logarithmically with frequency rank thus directly confirming the logarithmic cost-rank relation if the time of recognition is interpreted as cost. However this relation can be more naturally explained (at least qualitatively) by supposing that frequency of exposure to a word determines the speed of recognition, instead of vice versa, as is implied by the assumption that cost determines rank. If the "causal relation" is taken to be reciprocal, one is led back to the stochastic hypothesis in which no causal relations are assumed.

Nevertheless the theory is fascinatingly attractive, although to talk about its attractiveness would take us beyond the scope of analytical criticism of a scientific theory. However, the isomorphy of the theory with Boltzman's problem in statistical mechanics (to find the state of maximum entropy for a given average energy) deserves mention. For here is not only the much discussed (and much abused) analogy of information with entropy but also the analogue of energy (for the "cost" plays exactly that role) actually applies in a rigorous development of a theory dealing with the "steady state" of a language structure.

Most of all, the theory is attractive, because it introduces parameters which are in principle *interpretable*, something lacking in the stochastic theory. The average "cost" is such a parameter.

Another is B, whose reciprocal $1/B$ Mandelbrot calls the "informational temperature of the weighted vocabulary" because of the role its analogue plays in thermodynamics. As we have said, Zipf's law takes $B = 1$ in all cases. Departures are only anomalies. Here, however, B takes

[10] Mandelbrot derives the latter result first and calls it "the direct problem," then proceeds to the former result, which he calls "the inverse problem." We have reversed the order to pose the general maximization problem first, before passing to the special meaning of "cost" in the context of coding.

a range of values and strongly suggests a classification of linguistic samples, as does the value of m, the other parameter in (16), Mandelbrot's generalization of Zipf's law.[11] An important division seems to be between the cases where $B > 1$ (so-called "open" vocabularies) and $B < 1$ ("closed" vocabularies). Some idea of the sort of things which are revealed by the examination of the data in the light of the theory may be obtained from the following discussion by Mandelbrot.

> The great majority of weighted vocabularies are open. The range of variation of m is small, and $1/B$ increases with the intuitive wealth of the vocabulary.[12] In the usual samples of English and related languages, B varies around 1.2. But James Joyce's "Ulysses" has $B = 1.0$ (within error). At the other extreme, small children and schizophrenics show B in the 1.4 to 1.6 region . . . .
>
> Very few samples show $B < 1$ . . . , and those available to us are so special as to suggest that this state is avoided, unless R is so small that the maximum H compatible with $B > 1$ (which is $\frac{1}{2} \log R$) is too small, but the maximum H compatible with any B (which is $\log R$) is sufficient. For example, B was <1 for Modern Hebrew in the 1930's (it would be important to check whether it is still so). One also finds $B < 1$ in the foreign elements of mixed, but "metastable" weighted vocabularies, while the indigenous elements show $B > 1$. Example: English in Pennsylvania Dutch, as opposed to German, Latin in Notker's Mixed Prose, as opposed also to German. Besides, Yiddish, which is mixed but has not been studied in its components, shows one of the very few poor checks with the canonical law [eq. (16)]. All these cases suggest experimental checks upon our proposed explanation for the cases $B > 1$ [sic]. Another check would be given by the comparison of the B in normal children, and in children's books written by adults.[13]

Such suggestions reside not only in the parameters but in the entire "teleological" or—to succumb finally to more conventional usage—normative approach. If we suppose that under certain "optimum" conditions certain quantities tend to be maximized or minimized and derive from such assumptions certain theoretical results, the departure of empirical from normative results suggests that perhaps the "optimum" conditions are not met and that perhaps a search is in order for disturbing influences.

The challenge of Simon's stochastic derivation is difficult to ignore. It certainly has a sobering effect and serves an important function of making us look twice before we congratulate ourselves on discovering the operation of something more interesting than chance. (The philosophical view that all physical laws may be the gross manifestations of totally "undirected" interplay of probabilities is not relevant at this level of discussion.) But even if congratulations are premature, it would be unfortunate if a search for directed trends in human behavior (on the grand scale) were abandoned. As has been pointed out previously in these pages[14] the statistical structure of language seems the most promising field of investigating the operation of these trends—totally unknown to individuals, totally impervious to conscious influence by individuals but emerging in a harmonious grandeur on the scale of long-term human history, that is to say, as evolutionary trends as genuine as those that one finds in the biological world.

ANATOL RAPOPORT

[11] Zipf also attempts classification on the basis of the value of B. But his theorizing is marred by attempts to derive analogies on the basis of the same words used in different contexts. Thus he notes that the harmonic series $\sum_{r=1}^{\infty} r^{-B}$ *converges* for $B > 1$ (in the technical mathematical sense) and argues that therefore the language of schizophrenics, which, he notes, is typically characterized by high values of B is "convergent" in some vague semantic sense! (G. K. Zipf, *op. cit.*, p. 303.)

[12] It is impossible to resist the temptation to remark that here, at last, is a mathematical definition of "hot" prose in terms of $1/B$, the "informational temperature."

[13] B. Mandelbrot, "Simple games of strategy occurring in communication through natural languages," *Transactions of the I. R. E. Professional Group on Information Theory*, March, 1954, p. 135.

[14] R. W. Gerard, C. Kluckhohn, & A. Rapoport, Biological and cultural evolution: Some analogies and explorations, *Behav. Sci.*, 1956, 1, 6–34.

SIDNEY SIEGEL'S *Nonparametric Statistics for the Behavorial Sciences*[1]

Because of its practical value to researchers in the behavioral sciences the appearance of this book should be warmly welcomed, even if it does not make for exciting or challenging reading—but few readers would expect this of a statistics book anyhow. The task that the author set himself was to overcome one of the obstacles to the use of nonparametric tests listed by L. Moses in his review in the *Psychological Bulletin* a few years ago: that the presentations of these tests, together with their tables of significance, were spread out over the (mostly statistical) journal literature and thus not readily accessible to the potential users. A number of recent books on statistics or research methods have succeeded only to a limited extent in remedying this situation, by presenting arbitrarily selected tests and/or giving rather brief descriptions or references (e.g., Mosteller and Bush in the *Handbook of Social Psychology*, or K. Smith in *Research Methods in the Behavioral Sciences*).

In the present book, the author has pulled together most of the nonparametric tests developed in the last twenty years or so, and discusses them at length. The rationale and the formulae of the different tests are presented; computational methods are described in detail and always made explicit by means of the computation of a practical example. A summary of procedures, a short statement on whatever is known about power or efficiency of the test, and references to the original sources conclude such a description. In most cases, tables with values of the special statistics for given significance levels or p values for exact results appear in the appendix, so that a transformation to standard statistics like z or t is often no longer necessary.

In line with the much more practical than theoretical focus of the book, the organization imposed on this aggregate of tests is given by pragmatic considerations rather than by the inherent mathematical structure or relatedness of the various tests. They are grouped according to their applicability to experimental designs: we find a chapter on one-sample tests (i.e., comparison of sample values with a priori values), one for the two-independent-samples design, one for the case of two related samples, two chapters for k related and independent samples, and a final chapter dealing with nonparametric correlation measures and their significance tests. The other organizing principle used, within the gross structure of the experimental design, is that of the applicability of the test to the form of the data, i.e., the "level of measurement" required of the data in order to meet the assumptions of the test model. Tests for nominal, ordinal, and interval scale data are presented.

In view of the notorious measurement difficulties in the behavioral sciences, it is gratifying to see the number of tests which have been developed for data obtainable only in nominal or ordinal form *and* taking account of special problems, such as very small samples, large samples, and different types of alternative hypotheses. It is even more gratifying to find that the efficiency of these tests, in the case where a comparison with parametric methods is at all meaningful, is often not far below the parametric test. The temptations to apply the latter in situations where their legitimacy is in doubt should in the future be greatly reduced—especially since the results obtained not only gain in generality, but are obtained often with much easier computational methods.

The detailed description of specific tests occupies the larger part of the volume. Three introductory chapters are devoted to a discussion of the conceptual framework: the general theory of hypothesis testing, the concepts of significance, power and power-efficiency of a test, and a definition of levels of measurement. This section is concluded by a comparison of parametric and nonparametric methods and their relative merits (which, incidentally, is practically identical with Moses' evaluation). The treatment given to the theoretical part is standard, as

[1] *Nonparametric Statistics for the Behavioral Sciences* by Sidney Siegel. New York: McGraw-Hill, 1956. Pp. XVII + 316, $6.50.

found in many statistics textbooks of intermediate nature. The book presupposes only an elementary knowledge of statistics and very little mathematics, since derivations are only indicated in a few simple cases. Thus it should be helpful to the nonspecialist who wants to find some new tests applicable to his kind of data, and who does not want to deal with involved mathematics or even with moderately involved computations. Since the assumptions made by the nonparametric tests, besides being spelled out in the book, are usually exceedingly simple, it seems that—in contrast to parametric methods where this is debatable—not very much is lost if the user is not familiar with the derivation of the statistics or their distribution functions.

The book is well written, has a clear organization, and seems, in general, free from errors. A few points might be mentioned: the statement that "a one-tailed test is more powerful than a two-tailed test" (footnote 1, p. 11) may be confusing since it holds only in the given context. The conditions for the use of chi-square for the median test with total sample size < 40 (p. 112–113) could be simplified since the median break implies the determination of expected frequencies by the given sample sizes alone. And finally, the treatment of the significance level for the Wald-Wolfowitz run test (as well as the designation on the corresponding table in the appendix) seems to be in error since normally the alternative hypothesis tested is that of too *few* runs. Why the significance level (or the z value) should refer to two tails of the distribution is not clear (and at variance with the treatment by other writers)—especially since in the discussion of the one-sample case the possible one-tailed alternatives are pointed out.

The last remark indicates one undesirable feature of the book. For a systematic treatment, the organization around the structure of the experimental design, entailing the discussion of the same test for different cases in different chapters, with a minimum of cross references or even inconsistency (as in the instance cited), is somewhat awkward and engenders a feeling of lack of continuity. Together with the pragmatic orientation of the content, it seems to reduce its usefulness as a textbook. On the other hand, it probably makes the book more valuable as a reference for the practical user, since it presents clearly the alternatives available in a specific situation, pinpoints the test that would most appropriately be used, and explains in detail the steps the researcher has to go through in order to obtain his p values. The reduction in labor through the simplicity of required calculations and the included tables of significance should be appreciable.

From this point of view it seems that one of the methods that might have been included in the book is the chi-square approximation of Mood's maximum likelihood ratio, which by way of $n \log n$ tables can simplify tremendously the calculations of chi-square for tables with more than one degree of freedom. Theoretically, too, an indication that more complicated methods for the analysis of multivariate designs with nominal data are (or are becoming) available, (cf. M. McGill: "Multivariate information transmission" in *Psychometrika*, 1954) would have been desirable, even if a full discussion was beyond the level the book was aimed at. In general, this reviewer feels that a final chapter could have been added, giving a wider context, indicating an evaluation of the present situation and some of the theoretical issues and new directions in order to make the book more than a handy how-to-do-it for the researcher trying to catch up with recent developments in statistics. With regard to the latter purpose, however, the book certainly fills a gap in an important area, which should not be summarily dismissed, as McNemar in his statistics book does, with a four-page discussion and the statement that, in comparison to parametric methods, nonparametric tests are less powerful and consequently should not be used as shortcuts when the assumption of normality is tenable. What the use of analysis methods which can determine only whether a factor does or does not make a difference, without estimates of parameters, implies in the long run for the creation of a coherent theory might be an interesting problem for the methodologist to speculate about.

—FRANZ SAMELSON

ABSTRACTS OF CURRENT LITERATURE WITH INTERDISCIPLINARY IMPLICATIONS

Prepared by Members of the Staff of the Mental Health Research Institute

Belevitch, V. **Théorie de l'information et statistique linguistique (Information theory and language statistics).** *Bull. Acad. Roy. de Belgique, Cl. des Sc.*, 1956, 5e série, 42, 419–436.

The author develops a theory similar to, but in his view more rigorous than, that of B. Mandelbrot for the length frequency relationship between the words of a language. If a word is composed of i letters, let p_i denote its probability, or relative frequency, and C_i its cost. The actual values of the p_i's—according to the theory—will be those which maximize the ratio $M \equiv H/C$, where $C = \sum C_i p_i$ is the average cost of a word, and $H = -\sum p_i \log_2 p_i$ (different words of equal length i give separate contributions in the sums). The two basic assumptions are: (a) a linear relation between cost and length of a word, and (b) an equation of the form $N_i = an^i$ (a = constant) for the number N_i of words of length i that can be formed with n different letters. These are the two most general assumptions which lead to the frequency-length relation already found by Mandelbrot and verified experimentally by Shannon (the latter author's equation is a simplified form of Mandelbrot's). Shannon's work also allows the author to compare several parameters and predictions of his theory with experimental data. The agreement is satisfactory in most cases, and could even be improved by slightly readjusting certain values.

In addition, the theory is extended to account for the vastly different frequencies of phonemes in a language. Instead of being the length of a word, the index i in this case denotes the number of *distinctive features* which characterize a phoneme. These are "binary" in the sense that for a given situation they are important only by their presence or absence. The cost (in arbitrary units) of a phoneme is assumed equal to the value of i; the total number N_i of phonemes of cost i is put equal to a constant times the binomial coefficient $\binom{n}{i}$. Here n is the overall number of distinctive features, approximately one dozen according to Jakobson. The rank of a phoneme of cost i is defined as $\sum_{k=1}^{i} N_k$, i.e., the total number of phonemes with cost $\leq i$. Comparing this with the probability p_i determined, as before, by maximizing H/C, one obtains as a main result of the theory a relation between rank and frequency of the phonemes, which, in this case, turns out to be given by a sigmoid curve (in fact, the ordinates represent the cumulative frequencies of a binomial distribution). The experimental probabilities of 42 phonemes in the Russian language are compared with those predicted by the theory, again with good agreement.

According to the author, a similar theory could be applied to other elements of a language. For example, the distribution of frequencies with which the *letters* of an alphabet are used is also given by a sigmoid curve. This is shown by data which are available for the English, French, and German languages (also for Spanish, Italian, and Dutch, though much less accurate). (*ET*)

Boycott, B. B., and Young, J. Z. **Reactions to shape in *Octopus vulgaris Lamarck.*** *Proc. Zool. Soc. Lond.*, 1956, 126, 491–547.

Octopuses previously taught to attack crabs (their normal food) could be conditioned with the crab or an electric shock to attack or avoid appropriate geometrical shapes. Discrimination was obtained between large (8 cm) and small (4 cm) squares, vertically and horizontally placed rectangles, squares and rectangles or crosses, and certain figures differing only in shape,

not in outline or area. Somewhat less accurate discrimination was found between hollow and filled squares, squares rotated at 45°, and similar area-outline figures whose outside linear dimensions were not strikingly different. Discrimination between similar sized squares and circles was absent in two animals. Confusion with the large square did not result when the small square was presented at half the usual distance. An attack on a 4 cm square but an avoidance of 2 cm and 8 cm squares could be established, though less easily than when only two objects were used. Discrimination usually remained when the size of the test figures was reduced ½. The distinguishing attributes of the test figures were not adequately determined, though there was a tendency to attack the smaller figure. The total background of the presentation was important, and attacks on holder plus figure were significantly lowered when shocks were given with the holder when presented alone. A change in aquarium also impaired performance, but did not destroy the discrimination.

Urethane anesthesia was followed by a period in which crabs, but not geometrical figures, were attacked. Later, appropriate responses to the geometrical figures returned, though some impairment in performance was found if the previous discrimination had not been great or if an attack of the larger rather than the smaller figure was required. Much variation was present in the performance of different octopuses, possibly a result of previous experience or learning.

Following removal of the vertical lobes of the brain, positive test figures continued to be attacked. The negative figures also produced some attacks, but these could be reduced by further training. In the normal animal, octopuses learned to avoid crabs presented with an electric shock. This ability to retain memories preventing attack on crabs, in contrast to the test figures, was completely lost when the vertical lobes were removed.

These latter results are thought to indicate that different processes are involved in learning not to attack (1) a well-known food object (crab) and (2) an unfamiliar object (geometrical figure) not associated with food. The vertical lobes are apparently necessary for the former, the reversal of well-established positive responses, and may be involved in preventing attack on unfamiliar objects, but they are not necessary for the maintenance of these latter memories. (*DMM*)

Fitts, P. M., Weinstein, M., Rappaport, M., Anderson, N., & Leonard, J. A. **Stimulus correlates of visual pattern recognition: a probability approach.** *J. exp. Psychol.*, 1956, 51, 1–11.

In the first part of this paper the authors introduce what they call "metric figures" as an adequate stimulus for studying certain characteristics of visual perception. A rectangle is subdivided into $n \times m$ equal smaller rectangles, so as to obtain a matrix of n rows and m columns. To each column assign a number $\rho_i (0 < \rho_i \leq n;\ i = 1, 2, \cdots\cdots m)$, called "contour detail," and, starting from the bottom, fill as many of the adjacent small rectangles in the i^{th} column as the number ρ_i indicates. The role of rows and columns may be interchanged, and the matrix may be square. The actual numbers ρ_i are obtained by some sampling procedure; this justifies the claim to a "probability approach" to perception. In an $n \times n$ matrix the total number of figures that can thus be generated is equal to n^n, and the amount of information obtained in selecting a particular one of them—assuming equiprobability—is given by $\log_2 n^n$. If the constraint is imposed that no two contour details should have the same value (this corresponds to sampling without replacement), the number of figures is reduced to $n!$ and the corresponding information becomes $\log_2 n!$ The *redundancy* inherent in one of these latter figures is by definition equal to $1 - (\log_2 n!/\log_2 n^n)$. In a similar way relative redundancy is obtained by imposing other kinds of constraint on the sampling procedure, e.g., by requesting that the resulting contours should have some kind of symmetry. Visual *noise* to a given figure can be introduced by disturbing the regularity of its pattern.

Physical characteristics, such as dimensions of the cells, spatial orientation and brightness-contrast of the figures must be specified; their effects on perception can be studied together with those of the statistical properties. The authors point out that the low degree of meaningfulness is one conspicuous feature in this method of stimulation: metric figures are unlikely to evoke associations in the *S*s. The authors have begun an extensive program of research based on complexity, redundancy, and noise (together with certain associated physical features of stimulus patterns).

The second part of the paper reports two experiments as part of that program. In both cases the *S*s were asked to identify a given test-figure from among a set of metric figures, and the time required for recognition was used as performance score. This task "requires the type of visual discrimination that is the basis of man's ability to identify a particular pattern from among a group of similar patterns, but does not require that he name or describe it."

In the second experiment the effect of orientation of the figures was studied (i.e., each pattern was presented to the *S* in four positions: 3, 6, 9, and 12 o'clock). In the first experiment, on the other hand, other, mostly intrinsic stimulus factors, e.g., symmetry, were tested. We mention only the most significant result: figures constructed by sampling of contour details with replacement ("randomized sample," no redundancy) were identified much more quickly than those obtained from sampling without replacement ("constrained sample"). This is somewhat in contradiction to data by other workers on the relation between stimulus information and reaction time. The authors conclude that in their particular experiment the *type* of redundancy introduced was more detrimental to the *S*'s performance than the decrease of information per se. (*ET*)

Weiner, Herbert, and Schuster, Daniel. **The electroencephalogram in dementia—some preliminary observations and correlations.** *EEG Clin. Neurophysiol.*, 1956, 8, 479–488.

A certain correlation was noted between degree of mental change in patients and degree in brain wave change which suggested that the EEG might be useful as a diagnostic and prognostic tool. In order to verify this the authors studied 71 patients who showed dementia associated with a variety of clinical conditions known to produce diffuse, nonfocal brain changes. No regard was paid to age or sex. Dementia was defined as a more or less irreversible and generally progressive impairment of intellectual functions accompanying structural brain changes. The mentioned functions include the modalities of the sensorium such as attention, memory, retention, recall, calculation, and abstract thinking. The difference between dementia and delirium is seen by the authors in the reversibility of the latter while the former is progressive. Seventy-eight per cent of the patients with dementia showed abnormal EEG tracings which were essentially diffusely slow. A statistically significant relationship could be found between the degrees of dementia and the degree of EEG slowing. The number of patients with dementia who had also additional major disturbances of mood, thought content and behavior and EEG slowing was not greater than those who had only uncomplicated sensorial defects, nor could they be differentiated by EEG alone. The etiology of the dementia was not felt to be as important in regards to the EEG picture as the rate of progression and acuteness of onset of the disease. (*ER*)

About the Authors

Joseph Berger (On the Concept of Equal Exchange) is instructor in the Department of Sociology and Anthropology at Dartmouth College. He attended Brooklyn College and Harvard University prior to his instructorship. His research interests center around the formulation and the experimental testing of a model to describe the growth of power and status structure in small social systems, and special problems of measurement as they arise in the area of social mobility. The present paper (co-author, J. Laurie Snell) arose from a consideration of the concept of equilibrium. One of the authors had a concept of equilibrium which was based upon a notion of "equal exchange"; the other had a mathematical concept of equilibrium which was related to certain probabilistic processes. They believed that these probabilistic processes were applicable to the study of certain types of social phenomena—small group interaction, intergenerational social mobility, etc. After making the equal exchange concept formally precise, they were led to a stronger kind of equilibrium than the standard mathematical concept, and thus they were encouraged to develop a mathematical treatment of strong equilibrium. Specifically they were led to study the conditions of the process which would imply strong equilibrium and to study the properties of a system in strong equilibrium.

R. W. Gerard (Problems in the Institutionalization of Higher Education; An Analysis Based on Historical Materials) spent the academic year '54–'55 as one of the first group of Fellows at the Center for Advanced Study in the Behavioral Sciences. Before that, as Professor of Physiology at the University of Chicago, and later as Director of Laboratories at the Neuropsychiatric Institute of the University of Illinois, he was a "wet laboratory" scientist concerned with the nervous system and making only forays into problems of mind and behavior. (As a member of the Committee on Behavioral Science at the University of Chicago, he was perhaps the first person to bear the title, "Professor of Behavioral Science"). After his year at the Center, he moved more earnestly into these problems as a member of the new Mental Health Research Institute at the University of Michigan, and now, as he says, he is "both a water and air scientist."

This article formed from the confluence of these interests. The analysis was presented informally in the closing session of the indicated seminar and was later prepared, at Dr. Lazarsfeld's request, as the closing chapter of the Stern volume. We are grateful to Dr. Lazarsfeld for agreeing to its publication now as a separate article.

Albert A. Kurland, author of "The Drug Placebo—Its Psychodynamic and Conditional Reflex Aspects," received his medical degree from the University of Maryland and interned at the Sinai Hospital in Baltimore. After service with the Armed Forces, he accepted a position as staff psychiatrist at the Spring Grove State Hospital in Catonsville, Maryland, being appointed Director of Medical Research there in 1953. He is at present engaged in developing a program of research work which is attempting to explore the relationship between the psychodynamic and psychopharmacological factors in schizophrenic patients. As a result of an intensive drug research program and the fact that criteria for predicting whether a patient might display placebo reactivity were lacking, an extensive review of the literature and further attempts at experimentation in this area were made by his group. At the present time a group of lobotomized patients are being studied as to their placebo reactivity.

Jules H. Masserman (Evolution vs. "Revolution," in Psychotherapy—A Biodynamic Integration), Professor of Neurology and Psychiatry at Northwestern University, received his medical training at Wayne, Stanford, and Johns Hopkins Universities. Further psychiatric and psychoanalytic work was taken at the University of Chicago and the Chicago Psychoanalytic Institute. He is currently Psychiatric Consultant to the World Health Organization, the U. S. Navy, and the Veterans Administration, President of the American Association for Group Therapy, President-elect of the Society for Biological Psychiatry and the Academy of Psychoanalysis, and Chairman of the Section on Psychotherapy of the American Psychiatric Association. He is a

prolific and versatile writer, having authored three books, four monographs, and over 150 articles on neurophysiology, experimental psychology, psychoanalysis and clinical psychiatry. He is the Associate Editor of the annual volume *Neurology and Psychiatry* and Chief Editor of *Progress in Psychotherapy*. He has also been involved in the production of fourteen motion picture films, one of the better known being "Neurosis and Alcohol."

Rhona Sofer Rapoport, co-author of "'Democratization' and Authority in a Therapeutic Community," was born in Cape Town, South Africa and attended the University there. Her Ph.D. in sociology was received at the University of London. At present she is senior sociologist at the Belmont Hospital in Surrey, England. Past appointments have included Colonial Social Science Research Fellow, sociologist at the University College of East Africa, and Visiting Research Fellow at Radcliffe.

Robert N. Rapoport ("Democratization" and Authority in a Therapeutic Community) is social science research director at Belmont Hospital. He was born in Brockton, Massachusetts, attended the University of Chicago, and received a Ph.D. in social anthropology at Harvard University. Formerly he was social science field director for the Stirling County Study in the Epidemiology of Psychiatric Illness, and an assistant professor of sociology and anthropology at Cornell. He is particularly interested in the social sciences in the field of psychiatry.

Our congratulations and best wishes to Dr. and Mrs. Rapoport, since just prior to their article going to press, Rapoport requested that a change be made in the authorship listing since "by the time the paper is published we shall have taken some of our own advice and changed the underlying formal structure in the relationship."

Thornton B. Roby, author of "On the Measurement and Description of Groups," is currently Research Associate at the Institute for Applied Experimental Psychology, Tufts University, where his interests center on communication and decision-making processes in small, task-oriented groups. He received his Ph.D. from Yale University in 1950. His publications to date relate to theory and experiment on problems of small groups, and several collaborative articles on learning theory and personality. He states that "the background for the present form of this article was an attempt to analogize (rather freely) from Klein's celebrated Erlanger program, based on the relationship between certain types of invariance and the principal forms of geometry. It seemed possible that a similar structure, imposed on the data of behavior, might eventually provide a useful means of classifying and organizing such measurements."

J. L. Snell (On the Concept of Equal Exchange), Assistant Professor in the Department of Mathematics at Dartmouth College, was born in Wheaton, Illinois, and received his Ph.D. from the University of Illinois. He is co-author of *Introduction to Finite Mathematics*, (New Jersey: Prentice Hall, 1957), a book designed to introduce the student to some concepts in modern mathematics. He is also participating in the Dartmouth Writing Group, preparing for the Committee on the Undergraduate Program of the Mathematical Association of America experimental text material designed for a second-year mathematics course to be recommended for social science students among others. See also comments under Joseph Berger, co-author.

It was not so much by means of words I came to a complete understanding of things, as that from things I somehow had an experience which enabled me to follow the meaning of words.
—PLUTARCH

AN APPROACH TO HUMAN ETHOLOGY

by Claire Russell[1] *and W. M. S. Russell*

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This approach to human ethology is through synthesis of ideas and generalizations from lower animal ethology, psychoanalysis, cybernetics, and neuropsychology, which are relevant for a theory of human behavior. It emphasizes the relative failure of human intelligence, and its social derivative, science, to explore the social environment and refine social behavior. The synthesis, though limited in scope, provides a language in which, at one end, lower animal ethology, and at the other, art, science, and culture can be discussed in related terms. Certain pathological factors are considered, the control of which would speed up human evolution and release great potentialities for progress.

THE understanding and control of human behavior is beyond doubt, in practical terms, the most urgent problem of contemporary science. The wealth of observed facts in this domain is probably greater than in any other; a great number of disciplines and techniques converge upon the problem. Yet there is a notable lack of even the anlage of a unified theory of sufficient power and generality to coordinate all these approaches, above all to make effective use of the mass of observations on lower animal behavior which could provide an indispensable perspective. It would be absurdly presumptuous for any two workers to attempt the necessary synthesis in a completely general form, but in this paper we hope to bring together at least a few of the ideas and generalizations which must be combined into a coherent framework if all the relevant work that has been and is being done is to be directed with maximal efficiency to the solution of the key problems. In attempting this partial, or local synthesis, we have had in mind the findings of lower animal ethologists, the studies of psychoanalysts, the ideas of the cyberneticists, and the neuropsychological work of Halstead (26, 27). By the growth and fusion of many similar partial syntheses, we may hope to see the development of an increasingly general theory.

The first part of the paper concerns the relation between the venerable but vital concepts of intelligence and instinct, and their bearing on the pathology of human behavior, or, as it may properly be called, human ethopathology. It is from this aspect of our behavior that the urgency of the problem springs, and in the remainder of the paper we have made it the center of attention. In the interests of brevity, and in the belief that bold and clearly presented hypotheses are more urgently required than cautious qualifications, we present in both dogmatic and dramatic form a group of postulates which we believe to be of great importance. But while administering this shock therapy to the science, we have tried to counteract in a later section the impression of a monocausal attitude to ethopathology, than which nothing could be less helpful. Much, therefore, if not most of what we shall say would be the better for qualification, often drastic; having made this clear at the outset, we may proceed with less fear of misunderstanding.

The vigorous language of this paper when first presented[2] prompted the suggestion

[1] 43 Palace Court, London, W. 2.

[2] This paper is a somewhat expanded and modified form of one read at the Third International Symposium on Comparative Ethology held at

that we were making a number of value judgments, and it was felt that we were focusing undue attention on pathology, and not enough on the positive factors (e.g., in child rearing) in both the ontogeny and phylogeny of human behavior. These two valuable criticisms are related. Any value judgment about human behavior is a prediction about the span of survival of the human species, and vice versa; it is therefore impossible to say anything fundamental on this subject without value judgments, and we readily admit that these are notoriously subject to irrational influences. But these are all the more readily detected and isolated if presentation is frank and vigorous. It is *relatively* easy to make (correctly) a negative judgment; that is, to single out modes of behavior which, if they persist, will lead to fairly rapid extinction of the species. If every single human individual were paranoid to a psychotic degree, it might well be a matter of months. The converse, however,—to make positive judgments of relatively great certainty, that is, to single out modes of behavior which will make our survival extremely prolonged, in the last resort infinite, is plainly no joke; the difficulty increases in a simple way with the span predicted. It is like trying to pick an outsider from a very large field when most of the odds are astronomical. It is therefore hardly surprising that we have found it easier to describe and discuss "bad" behavior than "good." There is, perhaps, only one positive prediction anyone can safely make—that behavioral *variance* both within and between individuals must be conserved; and this is merely a particular application of a very old theorem (21).

Groningen in September 1955. In preparing the original paper we were greatly helped by discussions over a period of years with Mr. J. S. Hayes, and we should like to acknowledge our debt to him in the development of these ideas. The present version has benefited from a number of valuable criticisms and comments by Dr. M. R. A. Chance and by delegates to the Symposium, especially Dr. R. A. Hinde, Dr. P. Leyhausen, Professor K. Z. Lorenz, Professor Monica Meyer-Holzapfel, Dr. D. Morris and Dr. N. Tinbergen, to all of whom we are grateful. Finally, we wish to express our cordial thanks to Dr. R. A. Hinde and Dr. N. Tinbergen, who kindly sent us notes on their unpublished observations on parental behavior in birds, for permission to include these findings in the present paper.

INSTINCT, INTELLIGENCE, AND ETHOPATHOLOGY IN MAN

Any discussion of human ethology must begin with a consideration of the ways in which human behavior differs most markedly, if in degree, from that of all other species.

> "What is a man," asked Hamlet,
> "If his chief good and market of his time
> Be but to sleep and feed? A beast, no more.
> Sure, he that made us with such large discourse,
> Looking before and after, gave us not
> That capability and godlike reason
> To fust in us unused."

Expressed in terms of past beliefs and ideas, these splendid words take us nevertheless to the heart of the matter. Can we define more precisely "that capability and godlike reason" which, according to the Prince, marks us off from the other species? The term in use among scientists is the word "intelligence," and we must begin by examining this concept.

Early attempts at isolation and measurement of intelligence, whether conceived as one or several variables, contributed valuable advances in method, especially statistical method (notably the development of factor analysis) but failed signally to isolate a factor or factors with any clear biological significance. Thus the well-known Stanford-Binet IQ, despite its widespread adoption, is virtually unaffected by "chronic intermittent exposure to relatively high-grade anoxia" (26, p. 114) or by removal of both frontal lobes or unilateral removal of any other brain lobe (26, 31), and, in general, notably fails to accord, except possibly at its limits, with observations on the individual's behavior in everyday life.

The first successful approach to the problem of intelligence was made by Halstead, and its success was undoubtedly due to the fact that he started from the concept of biological adaptiveness. By means of well-chosen tests, applied to a wide range of subjects, including brain operates, Halstead obtained results which, when analyzed independently by Holzinger and Thurstone using

techniques of factor analysis, yielded four factors, whose significance he was able to demonstrate in extensive studies. All of them seem to be related to the cerebral cortex, and two of them (the *A* and *P* factors, see below) relatively specifically to the frontal lobes; these neurological findings are of the utmost importance when we consider the evolutionary history of the former structure in the vertebrates and the latter in the mammals, and suggest that the factors measured by Halstead are a valid first approximation to the specifically human "capability" of Hamlet.

The detailed nature of his tests (which were, of course, in large part deductively developed) enabled Halstead to describe these four factors qualitatively. First, there is the *A* factor—the capacity for abstracting universals or rational concepts, or classifying variables in the environment. In a typical test, the subject is presented with a series of stimulus figures, and must make his response by grasping "essential similarities in the presence of apparent differences and vice versa," abstracting principles such as size, shape, number, etc.

Second, there is the *C* factor, or "central integrative field" factor, or "organized experience of the individual," by which the inputs of learning are constantly tested against and integrated with the results of past experience. T. S. Eliot once pointed out that every time a new poem is written "poetry" changes, an excellent analogy (if not more than an analogy) with the proper functioning of the *C* factor, which should involve a constant modification of the *Umwelt* or *Weltanschauung* or set of models (19, 76) in the light of new information, analyzed with the help of the *A* factor. Whereas defect in the latter results in the formation of crude or meaningless categories, defect in the *C* factor may take the form either of failure to build any stable picture at all, or, a common finding in brain operates and diagnosed psychoneurotics, an excessive rigidity and failure to correct initial mistaken impressions. In a test where success or failure is signaled each time, and the subject high in *C* factor is able rapidly to change his tactics and abandon a mistaken principle for solving it, the rigid subject, having formed an initial hypothesis more or less arbitrarily, will stick to it in face of repeated error signals.

Rephrasing a little, we see that the *A* factor is associated with the capacity to discriminate between a great many slightly different situations, appreciating each on its own merits; the *C* factor involves:

1. Flexibility in correcting imperfect learning.

2. Free flow of information and central integration of all data acquired both by observation and computing processes. Information acquired in any context may be compared with information acquired in any other, and the whole complex organized before there is any question of overt behavior.

The use of these two faculties amounts to the supply of data to a mechanism which can then be used for prediction, and hence for elaboration of and choice between patterns of executive behavior. In the operation of these factors, a third one, the *D* factor, is called into play. This factor represents the set of particular modalities employed on both motor and sensory sides, which may be associated with focal areas of the cortex, e.g., on the sensory side responding to spoken or written language, and on the motor, speaking or writing (which may be differentially affected both in brain injury and functional disorder). We are concerned here with a set of executive mechanisms any of which may be called into play in the working of the two more general factors.

Finally, and of great importance to our argument, there is the *P* factor, or cerebral power factor. This factor has been related by Halstead to such fundamental properties as flicker-fusion frequency and the EEG rhythms, but these findings, which lead to a neurophysiological model, need not concern us at the present level. In behavior terms, he describes this factor in words which make it perfectly permissible to label it the *exploratory drive*. It is the process by which immediate gratification of needs is suspended while the environment is explored, the *A* factor utilized, and the *C* factor developed. Halstead sees it (naturally) in relative rather than absolute terms, as a factor which suppresses other drives in order to

permit exploration, in other terms, as a capacity to *tolerate* frustration and stress, for which a variety of *immediate* ready-made resolutions are adopted in lower animals (7, 15). "Must we not seek out a third factor [which turns out to be *P*] which serves to buffer these affect demands in the face of the psychologically 'new,' holding them in abeyance until the *A* factor can make its penetrations into the entropies of unstructured time and space?" (26, p. 69). Exploration can of course be made by overt motor behavior in the environment and also (at least in man) by a computing process in the brain in which various possibilities are explored without overt trial—the exercise of imagination.

The present review of Halstead's work cannot, of course, do it justice; but before leaving it we must note two further points made by him. First, the intelligence system is seen by him not merely as a mode of adaptation to present environments, but also as a mode of progressively increasing adaptability; and of course it frequently takes the form of adapting the environment in such a way as to give behavior freer play. "The biological view of the human organism must portray not only the present ecology of man but his potentialities for new ecologies, in short, his 'becomingness.' "

Second, we must note his view of the mechanism of attention, or consciousness, in relation to the *C* factor. He likens it to the tuning eye of a radio receiver, which can be set for particular ratios of *fidelity* and *discrimination*. The former implies a high "noise level" (5, 40). Thus, exploration may vary between fine analysis of a highly restricted problem and tentative surveyal of a broad reach of imagination. The point is that ideally *any* item of stored or incoming information should be *available* for attention or recall in relation to any purpose. Nothing should be inaccessible to the mechanism of attention (i.e., unconscious, in the strict Freudian formulation).

We may note in conclusion Halstead's view, which we fully share, that "neurotic modes of adaptation [represent] the polar extreme of biological intelligence," and that "biological neurosis can probably be duplicated in every essential detail without the requirement of brain injury, at least in the present meaning of this term, through learning (habits)." The remainder of this paper will be concerned to a large extent with the mechanisms by which intelligence comes to be thus stunted. But before we proceed to these, we must turn to the concept of *instinct*.

This concept, which we believe to be of nuclear importance, has been rendered almost unusable by its unfortunate liaison with the concept of "innate." The accurate use of the concept of phenotype, with its implied analysis of the nature/nurture problem, has been with us for decades, (for an exceptionally succinct recent exposé see Reference 54, pp. 360–361). But in the field of behavior endless semantic confusion seems to arise over the ethological concept of "innate," a simple practical category of mechanisms underlying behavior patterns which are not acquired either socially or individually by specific practice (36), in other words where a high degree of specificity is brought about by the general course of development in nonsocial surroundings adequate for physical survival and normally present in the natural environment of the species concerned (which at once excludes observations on mutilated animals, or e.g., rats reared in darkness [28]). This category may be more or less useful; it in no way conflicts with the proper use of the phenotype concept. But unfortunately in the fog of controversy and confusion "innate" has been equated with "instinctive." Since virtually no components of human behavior are "innate" in this sense, the term "instinctive" is thus disqualified from use by human ethologists. As a result attention is diverted from the immensely elaborate and fundamental similarities between instinctive behavior in lower animals and pathological behavior in man. One of our main aims in this paper is to spotlight these similarities, and we therefore enter yet again (cf. 30, 63) a strong plea that terms like "instinct," "instinct system" and "instinctive" be applied to all stable behavior mechanisms with the properties which we shall presently summarize, and which are abundantly illustrated in, e.g., Tinbergen's *The Study of Instinct* (70), *irrespective of their origin and*

mode of development.[3] While Lehrman (46) is certainly right in asserting that no classification of existing mechanisms in the adult absolves us from investigating their ontogeny, it is no less vitally important to examine the properties of mechanisms which, once formed, are certainly extremely stable, and this is in fact what ethologists have already done at some length. The justification for our usage will perhaps appear in what follows, but it may be mentioned that nearly all the properties of instinctive systems as described in Tinbergen's book can be paralleled in systems known to be acquired by deliberately applied conditioning processes (60). The cognate identification of intelligence with learning—still to be found, though less frequently—receives short shrift from Halstead. It seems likely, indeed, that conditioning in particular is precisely a learning mechanism designed for and suitable to an instinct system. In the light of what we shall discuss later, it is by no means impossible that irrational motivation is at the root of the surprisingly stubborn linkage of the innate/learned and instinct/intelligence dichotomies. Unpleasant issues could be evaded *either* by directing attention away from the *rigidities* of instinct mechanisms *or* by attending to these but stressing that these extremely unmodifiable determinants of behavior are not the outcome of stress in development but innate, and these are precisely the two courses adopted in the controversy. The confusion has not of course been universal; it has, for instance, been emphasized that the crucial difference between primate and bird learning lies not in capacity to learn (in which birds sometimes surpass primates) but in the primates' much greater capacity for rapid unlearning and relearning, in short, their greater flexibility (20, 25). We note at once a reminder of Halstead's control and operated subjects in tests measuring the *C* factor. Neurological guesses would be possible here (34, 77) but the crucial point is the emergence of a first important characteristic of instinct—*rigidity* or relative unmodifiability. "Innate" or "learned" or however acquired, the responses of a bird, a brain-operated human patient, or a neurotic human are slow to change.

In this paper we shall make an arbitrary division between man, as the exponent par excellence of the intelligence system, and all other animals as the harbingers of instinct. The distinction is of course oversimplified; elsewhere (60) an attempt will be made to consider the evolutionary process in its gradations; to relate, for instance, Halstead's *C* and *P* factors to work on familiarization in rats (16, 53). Here we note simply the supposition that the *P* factor evolved, in ethological language, as an *emancipated exploratory drive* (cf. the phenomenon of latent learning and the ideas of Thorpe [67]; also, Hinde [36]), no longer subordinated to, and exercised for the benefit of particular motivations, as isolated parts of separate appetitive behaviors. Many animals may explore an environment and modify their behavior when under the influence of one or other instinct motivation, e.g., when hungry or thirsty or when caring for their young, but the development of intelligence as an over-all factor involved exploration for exploration's sake, with the results available for any purpose. We submit that the complex of Halstead factors represents a new layer of integration, which uses the state of instinct mechanisms merely as information, and delegates to them, as appropriate, execution of any existing preset patterns, but may also initiate endless new ones. It is important to note at this point that the intelligence system should ideally explore and organize information not only about the current and previous environment, but also about the state of lower level central mechanisms, including what remains of the primary instincts. This is what may be called the capacity to become "aware" of one's wishes, and thus acquire control over them, with the ability to postpone the fulfillment of any of them in the long-term interest of the whole. (Cf. a suggestion by Le Gros Clark and Meyer, [17] about the function of the hippocampal circuit). The degree of this control represents tolerance of stress, in fact, the *P* factor level.

For what is this system substituted? We now turn to the properties of instinct systems, compared with those of intelligence.

These properties all hinge on the absence,

[3] Etymologically, the term "instinct" simply means "impulse."

absolute or relative, of the supercontrol system we have outlined, with its emancipated drive (*P* factor), appetitive behavior (overt exploration or imagination), and consummatory situation of the solution of a problem. The properties of instinct systems in lower animals which spring most readily to mind are those of *rigidity* and *isolation*. The former we have already mentioned; by the latter is meant the existence of a number of separate control systems between which there are only simple relations of stimulation and suppression, so that the behavior of the animal at any point is simply the resultant of their crude interaction. This property of isolation has many consequences.

Both properties may be illustrated by the case of mating behavior in the clawed frog, *Xenopus laevis* (57, 58). When a male of this species is placed with an ovulating female, he approaches her, clasps her round the waist, and remains clasping until after fertilization. When he is placed with a nonovulating female, he clasps her, unclasps again after a period of minutes, later clasps her again, unclasps again and so on. Clasping activity can be measured by the sum of the lengths of such clasping spells in a constant period, unclasping activity by the mean length of the spells. It has been shown that a variable (dose of chorionic gonadotrophin) which influences the former has no influence at all on the latter, and that, vice versa, a variable (the set of variables varying between individual nonovulating females) which influences the latter has no influence on the former. All this was shown by analysis of variance in the same experiments, and the demonstration of specificity was as convincing as could be for a conclusion involving a null hypothesis. There might, in short, have been two frogs in one skin, with no capacity for high-level communication, quite separately motivated and competing for the motor system.

Unclasping from a nonovulating female is the result, almost certainly, of repulsive stimuli given by the latter, which are not given by an ovulating female. Clasping a nonovulating female, therefore, does not offer the male any hope of the consummatory situation of fertilization, and in addition brings about its own eventual inhibition. It might be supposed that the length of clasping spells would progressively diminish over a period of observation. This is not found. Moreover, hormonally stimulated males were exposed 11 times, over a period of days, to nonovulating females for three-hour periods. There was no reduction in either mean spell length or total amount of clasping in a period, nor any quantitative difference between these males and others intermittently "rewarded" by being allowed to clasp ovulating females and hence attain fertilization. Males of this species, eternally hopeful, seem never to learn to take "No" for an answer (62).

This sort of extreme stability or rigidity is so common a property of instinctive systems that it is usually simply taken for granted. It is important to note that it may be shown in mechanisms which have undoubtedly been acquired by learning processes (20).

As for the isolation property, it essentially underlies the definition of the *releasing mechanism* itself (69, 70). It may be known from some experiments that an animal can undoubtedly discriminate certain external factors, yet in another behavioral context it may behave as though no differences existed. "For instance, the carnivorous water beetle *Dytiscus marginalis*, which has perfectly developed compound eyes . . . and can be trained to respond to visual stimuli, does not react at all to visual stimuli when capturing prey, e.g., a tadpole" (70, p. 25).

Similarly, the more general concept of *available* message set (63), and the whole analysis presented in that paper, was made useful and necessary by the property of isolated response to both external and internal factors typified in the *Xenopus* example above. Inputs available to one unit are not necessarily so to another.

Moreover, capacities acquired or deployed in the service of one instinct unit may be utterly unavailable to another. The finest example is still provided by Baerends' classical paper on the digger wasp *Ammophilia* (6), which can show surprisingly long memory in one context and virtually none in another. We have noted that there are, of course, evolutionary gradations, but it might be said that an animal with purely instinctive control systems is without the least vestige of a unified control apparatus.

Such an animal (if the pure form exists) is functionally virtually a colony of individual central nervous mechanisms, having in common the rest of the body. The "individuals" of the colony compete for the administration like so many political parties in a simple quantitative-vote way, and "governments" change repeatedly. (Cf., for instance, Van Iersel's study of the change-over in reproductive phases in the three-spined stickleback [39], or the function of the inspiratory and expiratory components of the mammalian respiratory mechanism, below the cortical level [55].)

In this system, various immediate and as it were *ad hoc* methods are present for meeting a situation which prevents a clear-cut election: a coalition government, alternation, election of a third neutral party, etc. The occurrence of displacement activities provides one means by which variance can enter the system on an evolutionary time base. Conditioning permits some facilities for change, but is severely limited by its close hook-up with individual instinct groups. Over-all change of the whole system is extremely slow, certainly phylogenetic (7, 30, 71).

So far the system may be contrasted with that made up of the *C* and *P* factors in man. What of the *A* factor? Certainly some capacity for abstraction is present in lower animals. But generally speaking the advance here has been in terms of greater generality and subtlety. For the instinct-ridden animal, any situation is already divested of much of its content by the limited mood of the animal, which dictates what shall and what shall not be noticed. The environment is then classified into a small number of relatively crude categories—red belly or no, and so forth. Both "relevant" stimuli and possible responses are few in number. A considerable amount of patterning is present ("innately" or by conditioning) on both sensory (the releasing mechanism itself) and motor sides (fixed patterns at various levels—stereotypy), and what there is of *A* factor has extremely limited play. Here again the stereotypy is associated with isolated control by a few instinct units and their associated moods (cf. Hinde, [35]). Intelligence therefore represents a capacity to discriminate a very much greater number of slightly different environmental situations, and a very much greater number of slightly different motor responses. This has of course been associated in evolution with the spectacular development of the mechanisms associated with the *D* factor, represented neurologically by the development of the sensory and motor cortices in the primates—specially connected, no doubt, with binocular vision and the opposable thumb. The outcome has, of course, been vocal communication and the birth of technology.

Isolation, rigidity, stereotypy, these then are the properties of instinct systems that distinguish them from the unified control system of intelligence. There is no doubt that the difference between the two extremes (there are no doubt all gradations) could be expressed with precision in terms of amount of information (74), directive correlation (66), or ultrastability (4). It is clear enough as a general conclusion that the intelligence system is associated with far more degrees of freedom of variance, or with a far greater amount of information. Perhaps it will be possible eventually to find optimal conditions for its operations; the analysis of Ashby is certainly of great value for the purpose.

Our central descriptive thesis is that pathological behavior in man represents a secondary reversion to instinctive modes of behavior—isolation, rigidity, stereotypy; the instinctive mechanisms concerned are to an overwhelming extent *acquired* by conditioning-like processes in development. It is in the *properties* of these mechanisms, rather than in their content, that pathology puts back the evolutionary clock. Neurosis thus acts to cancel out intelligence. Moreover, the pathological instinct-complexes have not even the merit of having been selected, like their counterparts in lower animals, as the best possible *particular* combinations, for the instinct systems in lower animals, however rough and ready, however they may break down in unusual conditions, however readily the ethologist can lure their possessors to commit absurd solecisms, however they may limit the colonization of new environments, *do* manifestly work in the restricted conditions for which they were evolved. Halstead equates his intelligence system, i.e., all four factors, with the

Freudian ego; we may similarly identify these pathological acquired instinct formations with *both* the Freudian superego *and* the Freudian id. These and similar subsystems tend, as Freud showed, to be played against each other in the desperate attempt to establish a balance which will permit some limited degree of freedom for the operation of intelligence. In fine, we may make the following descriptive generalization:

Ethopathology in man is the study of factors that make his behavior effectively less intelligent and more instinctive. This descriptive and comparative statement could be supported by a great wealth of detailed parallels. However, we shall now consider the *manner* in which what we gain on the swings of intelligence is lost on the roundabouts of neurosis (using that word in its wider connotation, to include, for instance, delinquency), and therefore, in particular, the development and vicissitudes of the intelligence system in the growing child. For as the whole of the positive achievements of mankind turn on the working of the exploratory drive, so all that is wrong with his behavior turns on the exploratory drive's malfunction.

DOMINANCE, SUBMISSION, AND SOCIAL INTELLIGENCE

Before we begin this analysis, the argument must be carried one step further. Intelligence in the individual is associated with social communication, with technology, above all with science and art. And it needs little reflection to see that, as a species, we seem so far to have used our intelligence mainly on the nonhuman environment. Technologically, we have been capable of prodigies of exploration, refinement, virtuosity. But in our dealings with each other, the wonderfully varied and versatile possibilities of constructive social intercourse have been all too little attempted. History, when it deals with social relations, is indeed a record of crimes and follies. It is incredibly repetitive by comparison with the history of physical science and technology. One sees a constant recurrence of certain basic social patterns, pursued indeed through all their limited variations. We can be more specific. Social relations have tended to an overwhelming extent to stay in the well-worn grooves of the system of *dominance and submission*. These can be expressed in many forms, but there is a terrible sameness and rigidity about them. Summing up, we may say that at the social level, in dealing with each other, our effective intelligence has been extremely limited: something seems to have blocked our capacity and drive to explore the potentialities of this field. Always, there is a subsidence into the dominance-submission pattern, as for instance can be seen after every great political revolution, beginning, perhaps, with a genuinely new social idea, but almost at once relapsing into a rat-race, or, as a rodent might more justly call it, man-race.

This relative specificity of the block appears elegantly at the individual level in some experiments of Halstead (27). As we have mentioned, he had found that neurotic and brain-operated subjects showed considerable impairment of intelligence as compared with subjects classified as "normal" or "healthy," when all were tested with purely nonsocial problems (geometrical visual patterns, etc.). But when he used as test objects a number of outlines of human faces, which the subjects were required to classify as gay, sad; beautiful, ugly; likable, unlikable; etc., he found that "our normal individuals are just as irrational as our brain-injured individuals or as our psychiatric patients." The irrationality showed itself in acceptance and maintenance of patently unjustified stereotypes. In other words, the great majority of humans, though regarded as "normal," show instinctive modes of response in *social* situations; the diagnosed "neurotics" merely show a further stage of more generalized degradation. It is the social situation that strains the intelligence mechanism, and this, of course, provides a fundamental clue to the origin of the disturbances and distortions.

Since we have introduced the concept of dominance, a few words are necessary to clarify its significance in man. First, we may note that with the onset of social cooperation on tasks, dominance acquires a new dimension. In lower vertebrates, including the primates (14, 15), dominance is mainly *competitive*; that is, the effect of the more powerful animal is to *prevent* the less powerful from carrying out its appetitive be-

havior, and threat is exercised with purely negative results. With the dawn of cooperation, *exploitive* dominance becomes possible, that is, the forcing of another individual to do one's own appetitive behavior. Possible origins of this in lower animals may perhaps be seen in the interesting work of Littman *et al.* (48). This should, however, not be confused with what we may call constructive *leadership*, a division of labor in which the actions of one individual are directed by another, who is equipped with a greater amount of information, the whole being done in the interest of both. A whole spectrum of social relations can now come into play. Essentially, the notion of "passive" as it appears in psychoanalytic literature is related to the situation of the dominant exploiter; a great deal of confusion has arisen from the equation of the active/passive dichotomy, an extremely complex and pathological social interaction, with such simple and venerable relations as the role of the two sexes in sexual behavior.

It is important further to note that social communication, which must have begun on the human scale at about the same time as the extensive cooperation which it made possible, has from its outset been partly prostituted to dominance uses. The essence of cooperation is the free and adaptively directed interchange of information between individuals, chiefly by spoken and written language. Dominance, however, is attained by withholding information from others, by deceiving them with "false" information, by confusing them, and in general by inhibiting the free play of their exploratory drives and their capacity for accurate prediction of the behavior of others. This was clearly perceived a long time ago by such accurate observers of dominance behavior as Machiavelli and Talleyrand, and with such modern developments as that of game theory, approaches the status of a fundamental theorem. Although the comparison of individuals with populations has been beset with pitfalls since the *Republic*, it has recently been used with some success when the individual's brain is considered as a population of central nervous processes (56). It is therefore worth noting that the effect of dominance activity at the cultural level is to split human society into units between which little information passes, and whose relations are governed by crude interactions —a very pretty analogy with the instinct as opposed to the intelligence system. Here, however, let us simply note the crucial fact that dominance is attained by inhibiting or perverting the exploratory drives of others, for this will prove central to our hypothesis.

What, then, is the specific block on exploration of social possibilities in man? This is what we are to consider.

ETHOGENIC INHERITANCE

The scope of ethopathology can be summed up picturesquely in a single bold—or bald—sentence, as *the study of the relations between the behavior of the individual and that of his parents.*

This misleadingly simple formulation must of course at once be qualified in several ways. First, we do not wish to start another sterile controversy about the relative effects of genotype and environment. Of course, there must be genetically determined central nervous differences between individuals, notably in respect of the four intelligence factors. In lower animal behavior ethologists have begun with "innate" mechanisms. But in considering man we must, on the whole, reverse the process. Until we have fully disentangled the influence of the parents' behavior on the growing child, genetic studies of human behavior will be beset with pitfalls, except where the characters studied are relatively crude, macrostructural, or macrophysiological defects, or where monozygotic twins are fortuitously separated virtually at birth and brought up in totally different surroundings. For similarities between the behavior of parent and child can be generated by reactions to the parents' behavior, itself in turn similarly a reaction to the grandparents'. These similarities may be of great specificity, and in their transmission through the generations they can ape the processes of segregation, recessiveness, and so forth; and the unwary might readily assume them to be genetically determined. So our first study must be that of *behavior* interaction through the generations, the process dimly glimpsed by the Greeks in the primitive concept of the family *Ate*, or curse. It is noteworthy that while the curse in Greek myths is usually voiced

initially on the whole house with all its descendants, it is usually implemented by successive cursing of child by parent down the generations, as is strikingly illustrated in the *Oedipus Coloneus*.

The strikingly close relations between the whole life histories of individuals and those of their parents or other relatives become clear, once the pattern is grasped, in any cases the observer chooses to study from this point of view. The *patterns* relating the life histories are of course often varied and complex. When a number of cases have been considered, however, it will be seen that the illustration we now give is not quite as far-fetched and fantastic as it at first sight appears. It is related of Napoleon that his mother, a Corsican country woman of narrow mental horizons, had one ambition for her son—that he should be a land owner in Corsica. When the Emperor was at the zenith of his career, and had occupied most of Europe, the old woman seems to have failed signally to grasp the scale of his achievement. It need only be noted that Napoleon ended his life as a land owner on a small island (cf. 11).

We must, of course, immediately qualify the word "parents." The development of an individual's personality may be influenced by the behavior of every individual with whom he comes in contact throughout his life, and his behavior at any time is of course also partly determined by the immediate social environment. Nevertheless, we have every reason to suppose that the effect of social stress falls off exponentially, or in some similar way, as a function of age. An infant or child exposed to relatively mild stress may be supposed to be affected as drastically as an adult exposed to very intense trauma indeed. We have, of course, no quantitative data on this point, but the mass of findings by Freud and his successors fully justify the contention that early childhood is relatively crucial, an observation that clearly relates to the course of development of the intelligence system, which takes some time to attain an adequate degree of organized stability (in particular, this clearly applies to the *C* factor). Even in childhood, the individual may be in the charge of relatives or guardians other than his parents, and even the child brought up by his parents is usually exposed in varying degree to other relatives or, in some social strata, to nurses, etc. (Siblings also of course play an important role, but since reactions to them are usually secondary, in the sense that the child deploys in relation to them behavior patterns acquired in relation to the more powerful adults, we have ignored this complication here). Nevertheless, when all this is said, it remains true that at least in the "civilized" cultures of to-day the behavior of the two parents is crucial, and it is on this that we have concentrated. We may add that since the child is usually exposed more directly and continuously to the mother in earliest infancy, her behavior may often be critical. The fact that the infant was exposed chiefly to a woman at a period when his or her intelligence was particularly rudimentary, and the environment therefore seemed particularly vague, arbitrary, and uncontrollable, has led to such social stereotypes as the mysterious unpredictability of *das Ewig-weibliche* and been reflected in the peculiar terrors of matriarchal religions. Father comes on the scene more when the child is acquiring some grasp of an ordered environment—hence the law-giving, methodical gods such as Zeus uneasily superimposed on the unpredictable mother goddess. We can similarly trace here the origin of the endless and sterile philosophical discussions of the relative roles of chance (mother), predestination (father) and free-will (child). (The metaphysical muddle can be analyzed readily in terms of prediction and dominance, but this we shall not essay here). In fine, we shall concentrate on the role of the parents, bearing in mind that that of the mother is often critically important.

Third, we may distinguish broadly, though all gradations exist, between the overt and covert behavior of the parents. By overt behavior, we mean the more obvious day-to-day behavior with which the parents themselves are fairly familiar, and which they would recognize as their own if pointed out to them. By covert behavior we mean the partly unconscious attitudes and mood states of the parents, determined by drive and phantasy conditions of which they are by no means aware, and which they would indignantly repudiate. Such

central nervous conditions in the parents will, however, be expressed in two ways. First, they may come to a head and be vented in an occasional outburst of, for instance, attack behavior, which will be for the child what is called a traumatic situation. Second, and more insidious, such motivations will produce subtle effects on the parents' behavior at all times; expressed in minute intention movements, they will impinge on the child or infant as a thousand small daily cues, as Lorenz has shown in the case of dogs (51). These indications of his parents' covert attitudes and wishes will have profound effects on the child's own behavior. Lorenz's dog Tito is an admirable illustration here, for he has described how she sensed his own wishes and carried them out. In that instance, he was aware of these wishes, but few people are so able or honest in their self-observation, and most parents are surprised by behavior in their children which is just as directly dictated by their own as Tito's was by her master's.

The malfunction of parental behavior

We must begin, then, by considering the behavior of human parents. And here, we shall inevitably tend to concentrate on pathology. It is possible to build up a conjectural picture of adaptive parental behavior somewhat as follows: after a number of experimental relationships (no doubt varying inversely in number with the individual's capacity for imagination), a man and woman with suitably matching personalities establish a relationship from which each derives maximal satisfaction, in bed and out. They are aware that they would enjoy cooperating in the rearing of children, and that they would derive enjoyment from their relationship with them; in due course, therefore, when they can provide optimal conditions for rearing, they produce children. Having an optimal relationship with each other, their attitude (an attitude is simply that which generates given moods in response to given situations) to the children will be purely parental; that is, they will derive pleasure from seeing the children happy and developing greater and greater independence. This picture is clearly elusive and imprecise. We could sharpen the language considerably, but shall not attempt to do so. What is clear is that such adaptive behavior is virtually impossible to characterize in detail, and cannot be reduced to a set of rules or precepts; variance and high directive correlation are of the essence. It is beyond question that such pure parental behavior must be present in many families to a considerable extent; otherwise the human condition would be immeasurably worse than it actually is, and, in particular (as will be clear later), no scientific advance could ever have occurred. But the description of that behavior by parents which has a beneficial effect on the personality can only be done at an extremely general level. By contrast, it is all too easy to pinpoint those maladaptive features of behavior by parents which certainly do have deleterious effects. This preamble is necessary to avert misinterpretation of what follows, which might otherwise seem to involve a sweeping condemnation of all parents. We shall deliberately sharpen our outlines by considering severely disturbed families; we must nonetheless emphasize that, albeit in very varying degrees, something like what we shall describe probably happened to every one of us, and that as yet no individual is immune to effects of these kinds.

In both directions, everything depends on the relationship between the two parents themselves, and the generally sound conclusion is that the happier they are together, the better for their offspring. To the extent this is *not* so, the relationship between them *must* be a balance between dominance and submission, in which neither enjoys full satisfaction of natural appetites. All those inter-instinct processes which ethologists have observed in acute forms in lower animals will now come into play on a chronic time scale. Since both parents are thwarted in respect of certain natural wishes, and unable to apply their intelligence to finding a more satisfactory situation, there will be displacement activity (44, 68, 71, 73) and redirection activity in the sense of Moynihan (7). Both will be directed onto the victim readiest to hand and most at their mercy, namely the child.

Thus a sexually thwarted mother, for instance, will redirect her sexual appetitive behavior on to her small son (or daughter). In some cases the seduction of the child may

be quite conscious and deliberate on the mother's part, and may take the form, for instance, of inducing it to share her bed and stimulate her in various ways. Now there is no conceivable need for the sex drive in the child to anticipate the mechanism of puberty. While infantile sexuality is an observed fact, there is no reason whatever to suppose it is ever autochthonous. To some extent, as one of us, (C.R.), has suggested (30), it may arise by displacement, itself the result of maltreatment of other kinds. But premature arousal by one or other parent is perhaps an even more crucial factor. Now it is known even in rats (41) that prepuberal exposure of males to receptive adult females produces lasting distortions of their sexual behavior. Moreover, the covert atmosphere is normally one of dominance and threat. The acute distress of the experience on the child's side is vividly expressed in the *Hippolytus* of Euripides. (Racine was more concerned with aspects of infantile experience which could be conveyed in the person of Phedre). Indeed the plays of Euripides express in general with some degree of insight (though of course partly in phantasy terms), the particular terror which may be caused by the behavior of a mother. No doubt this resulted in his reputation as a misogynist.

The seduction of the child may in other cases be the result of a quite unconscious redirection on the part of the parent. In this case, any response on the child's part will be brutally punished, and for the rest of his life his sex drive, prematurely aroused in conditions of both threat and repulse, will be grossly disordered.

The dominance-submission relation between such parents is one in which, on both sides, fear and rage are uneasily balanced, and each parent may displace or redirect both attack and revulsion onto the child. Both aggressive behavior to children and irrational fear of them may readily be observed in many situations. Finally, since it is always found that neither parent wholly dominates the other in all spheres of activity, each will attempt to dominate the child —a singularly inappropriate reaction—and each will attempt to interpose the child between them, and use it as a weapon in the continual covert intrigue. In fine, a human child may be for its parents an object for sex, attack, fear, disgust, dominance, and even perhaps predation.

Nonparental responses to young in lower animals

In this context it clearly becomes of importance to examine nonparental reactions (e.g., sexual, aggressive, etc.) of lower animal parents to their own young. Observations on this subject, though doubtless not infrequent, are scattered in the literature, and a systematic review would be of the utmost value. Here we shall confine ourselves to a few salient observations and ideas.

Animals which show no parental care naturally tend to display other than parental behavior toward their offspring. For both males and females of *Xenopus*, for instance, eggs and tadpoles, including their own, are treated simply as food. In species with uniparental care, the nonrearing parent (e.g., the female in sticklebacks) may again have a purely predatory response to eggs and young. We are concerned here only with the responses of parent animals of one or both sexes which do, in general, adopt a parental pattern of behavior to their young.

Inappropriate reactions to the young, especially aggressive or predatory, are of course frequently found in parent animals in such artificial conditions as captivity, but it is known that such conditions distort all forms of social behavior (32, 33, 49). Hediger gives a neat example in his more recent book (33). A male zebra who showed great excitement on seeing his mate with their recently born offspring was allowed in the same paddock with them. He suddenly attacked the young zebra, and was with difficulty driven off. Hediger tentatively suggests that the zebra had mistaken his own young for a predator, and was simply carrying out his protective parental function in an unfortunately misguided way.

Observations in the field, or in near-natural conditions, do not seem to reveal, at least commonly, such drastic aberrations, but intention and low intensity attack and threat movements directed against the animals' own young are not uncommonly observed in birds; also low intensity escape

reactions.[4] Tinbergen has seen attacks on (own) young in Herring Gulls, especially (*a*) when small young are a little distance from the nest, and (*b*) when young are nearly full-grown, and particularly when they then pester parents for food. Attacks on large young are inhibited by the latter adopting the submissive "hunched" posture. Tinbergen thinks the explanations for attacks may vary for different situations. Attacks on small young (which may also occur in Little and Common Terns) may, he suggests, have to do with "incomplete arousal of parental drive which allows a hostile reaction to strange things in the nest to break through." As for attacks on large young, he suggests two not incompatible possibilities: (*a*) overlap in the parents between the last stage (parental) of one cycle and the first stage (aggressive) of a new cycle, and (*b*) the fact that the young progressively loses chick properties and assumes those of the adult, hence more readily releasing an attack drive (normally directed onto rivals) which has been present throughout.

Hinde's observations on Passerines (*Parus major*, *P. caeruleus*, *Fringilla coelebs*, and *Chloris chloris*) are strikingly similar. He observes attack and escape reactions to own young in all four species; in the first two "such responses are especially likely when the young are begging very vigorously." As the young become partially independent of the parents (*Parus spp.*) aggression by the parents becomes more likely. Hinde makes the interesting suggestion that "juvenile plumages may be important not only because they are more cryptic than those of the adults, but also because they lack those releasers which evoke aggressive behavior." He also notes that the begging posture in young after fledging, "when the birds are feathered and resemble the adult more closely," differs from that used immediately after hatching. This later posture, resembling the adult female soliciting posture, contains submissive elements, and Hinde regards it as less likely to evoke aggressive behavior.

It is apparent from these concordant accounts that inappropriate reactions to young are prevented or damped in these species by linked adaptations in both parents (suppression of aggression in the parental phase) and young (juvenile plumage and later adoption of submissive postures as they become more likely to evoke attack). Thus the young will not normally evoke aggression until they are able to inhibit this by submissive postures. (The attacks on small young, which in all three species mentioned by Tinbergen consisted in pecks directed at them when away from the nest, may perhaps have been components of parental behavior proper, designed to keep the chicks in safety, and not hostile at all).

On the parent's side, aggression etc. seems to be suppressed for at least the early part of the rearing period. Further light is thrown on this by Tinbergen's observations on the Eider Duck. Females of this species "who have no chicks of their own . . . are strongly attracted to females whose eggs have just hatched. Such 'aunts' walk up to chicks whenever these move a little away from the mother, but when the chicks walk then towards them, they show signs of both fear and aggression: they sidle away, and often peck at them. From what observations I have I got the impression (but no more) that the mothers themselves show slight signs of aggressiveness, and my suggestion is that we see the overt attack in the 'aunts' because their parental tendencies are slightly lower than those of the mothers, and not entirely strong enough to override aggression completely." Further clarification comes from endocrine studies. By repeated injections of prolactin into ring doves with previous experience of breeding, Lehrman (47) caused them to show effective parental feeding behavior toward seven-day-old squabs, "very active, but not yet able to leave the nest," which were the offspring of other birds. "The differences in success in feeding the squabs were by no means the only observed differences in behaviour" (between the prolactin-injected birds and water-injected controls). Specifically, the control birds displayed a considerable amount of both courtship and aggressive behavior

[4] We are deeply indebted to Dr. R. A. Hinde (37) and Dr. N. Tinbergen (72) for notes on their unpublished observations and conjectures in various bird species. Sent to us independently, these notes show striking agreement on several points.

toward the squabs, while the prolactin-injected birds did not, at least to any marked extent. It is striking that the same difference in respect to courtship and aggression movements was seen between prolactin- and water-injected *inexperienced* birds, although Lehrman's experiments showed that experience of breeding is crucial for success in the positive aspects of parental behavior (feeding the squabs). It is clear that, at least in this species: (a) Suppression of sexual and aggressive behavior to the young is brought about by a hormone. (b) This suppression works for any young, and is not specifically developed in relation to a bird's own young. (c) The suppression is independent of breeding experience. Lehrman, in the light of earlier work, interprets the suppression in his observations as due to the anti-gonad effect of prolactin—in other words, to reduced release of gonadal steroids which normally facilitate courtship and aggressive behavior.

In lower vertebrates, therefore, parental behavior is probably kept free of contamination with other types of response partly by structural and behavioral adaptations in the young, and partly by the separation of the reproductive cycle in adults into phases by endocrine control.

Now hormonal influences on behavior are of two kinds—initial maturation of behavior mechanisms, and reversible facilitation of the mechanisms once established (10). It is known that, while hormonal maturation may still be important in man, hormonal facilitation of reproductive activities becomes less and less marked up the primate series, and is probably quite negligible in man (9). The role of the sex hormones is increasingly, and in man overwhelmingly taken over by the cerebral cortex.[5] In man, therefore, we must suppose the control of parental behavior in its negative aspects to be largely if not entirely central nervous; it is thus at once exposed to every kind of behavior disturbance. We might suppose that the only appropriate signal for starting to rear young (in a species which has ungeared sexual activity from reproduction by various devices) would be the realization of having found a thoroughly satisfactory partner. If children are born and raised before this is attained, the redirections and displacements we mentioned will necessarily ensue, since, in effect, the parents are still in a preparental phase of behavior. This obtains whether or not they realize it, and whether or not they express it overtly by continuing to have affairs outside the relationship.

Blockage of the exploratory drive: stereotypy and rationalization

Now let us consider the situation from the point of view of the child. We suppose that if parental behavior were optimal, the child would only gradually begin to mature, develop, and give play to his[6] exploratory drive, as a means to developing independent modes of behavior by the continuous exercise of the *A* factor in expanding the organized field of the *C* factor. For initially all his needs should be supplied by the parents in response to very simple appetitive behavior on his part. Against this secure background, the child can begin to explore at leisure, and so prepare for independent life, and this process should be encouraged by the parents. But now consider what can go wrong.

First, the child's needs in infancy are never as fully gratified by the parents as they could be, since the latters' parental behavior is impaired. Further, when one or both parents begin actually to threaten and behave in a hostile manner, however covertly, the child is forced by the urgent motive of fear to explore, and in particular

[5] It is noteworthy that even in lower mammals correlation has been shown between sexual activity and various measures of learning ability (2). In man, there is reason to suppose some degree of correlation between sexual activity and what may perhaps be Halstead's *P* factor (cf. 43). This correlation did not escape Freud, but unfortunately he expressed it in terms of the extremely misleading libido concept. Since the correlation, if true, clearly reflects the dependence of both types of activity on cortical facilitation, Freud's "energy" or "hydraulic" type of concept, which implied a source of energy which must be divided between two tasks, was peculiarly inept: it logically leads to the deduction that the two activities should be overtly *negatively* correlated—the exact opposite of the truth. Hence the notion of sublimation, which is simply a rationalization for avoiding sexual activity.

[6] For convenience, only one gender is used throughout.

to explore the social situation and its possibilities of acting within this. Thus exploration begins prematurely.

We must further recall that the parent wishes to dominate the child. He or she therefore wishes to prevent the child from exploration and accurate prediction, and further, to perpetuate the state of dependency which puts it in the parents' power: this can be done by denying it the opportunity for developing independent modes of behavior, and imposing upon it patterns of rigid behavior so compulsive in their effect that they effectively perpetuate the child's early environment and keep it helpless.

Now the data needed by the child for deciding what to do are of two kinds: first, he must observe and record his own wishes, state of mind, in general and to speak precisely *mood*, in a sense common in ethological usage and which has elsewhere been defined with precision (63). The second kind of evidence he must observe is the mood of the parent or parents he is dealing with, inferred from their behavior in both crude and subtle aspects. But in examining these impressions, the child is investigating something extremely unpleasant. The parent, on whom he utterly depends, and whose friendly behavior is seen as a condition for gratification and even survival, is hostile, threatening and frightening! His own reaction or mood is one of fear of someone he should never be afraid of. Of course, we do not suppose that the infant puts the position to himself in these terms, but the effect may be expressed as an intolerable conflict or state of ambivalence. Expressed in communication terms, exploration consists essentially of the presentation of a body of noise, or random excitation, to a filter mechanism which imposes upon it some degree of organization (5, cf. 40). In the present instance, the filter is very imperfect and rudimentary at this stage, and the noise represented by this contradictory set of impressions is overwhelming. In the central nervous system, the concept of noise is related to that of pain or distress, the characteristic of even peripheral pain fibers being the small amount of information they carry. And to see the thing vividly, we must imagine the exploration the infant is carrying out to be intolerably stressful or painful. Meanwhile the parent is, as we noted, in one way or another seeking to dominate the child and force upon him a rigid pattern of behavior, above all to inhibit his exploration and prevent him from developing independence in his behavior, since that is incompatible with submission.

In these conditions, several effects are produced. First, the child, finding exploration intolerable in itself and irritating to the dangerous but necessary parent, inhibits his exploratory behavior and readily accepts as a solution the rigid form of behavior forced upon him and in various ways suggested to him by the parent. Thus, the first effect is one of submission, expressed in the adoption of a rigid and stereotyped mode of behavior, through some kind of conditioning process.

The notion of submission in this context is a subtle one. We have seen that young birds inhibit their parents' attacks by means of submissive postures. But there are two important differences in the human case. First, submission must be adopted at a very early stage in development, whereas in the birds it is only required when they are becoming independent. Second, there are no simple, standardized postures which will inhibit the human parent's threat. Instead, a pattern of submission is required at a much more general and high level of behavior, permeating the whole system. Submission, in birds a simple posture, assumed when needed, means for the child a continuous and to some extent variable and adjustable conformity with the complex patterns of behavior which the parents, for their own irrational motives, wish to impose; an adaptation, in short, to their phantasies. Tinbergen suggested to us the useful formulation that parental *approval* becomes for the child a universal consummatory situation for all behavior; and approval means simply cessation of threat or other negative attitudes to the child. Therefore the child's submission requires further scrutiny.

The central feature of it lies in the rigidity imposed, the *compulsive* nature of the behavior at the time and in future whenever the individual encounters crude key stimuli to which it is now conditioned and geared. It by no means follows that the behavior *looks* submissive in a naive or obvious sense. It may take the form of a compulsive

rebelliousness. It may even take the form of overt domination of the parent, as in the well-known phenomenon of the "spoilt" child, who grows up a delinquent. The crucial point is that a whole area of the individual's behavior has been rigidly molded to fit in with, or be adapted to, the complementary behavior of the parent. At first sight this notion of forcing a child to dominate seems paradoxical and curious. The idea can be illustrated rather vividly from a court case a few years ago in England, in the course of which it emerged that a certain woman was forced by her husband's threats to tie him up and beat him. Perverse quasi-sexual phantasies and practices of this kind are always found on analysis to sum up and vividly express processes which are at work all the time in a much less obvious way in ordinary social intercourse, and close observation will often show this process at work in everyday life. The parent of the future delinquent sees in him a stimulus for submission; but submissive behavior in the parent is just as inappropriate and terrifying to the child as any other aberration. In this context, it might be mentioned that this sort of behavior is often nowadays rationalized as "permissive child-rearing based on modern psychological findings" etc. The public, though confused on the matter, is rightly suspicious of this pseudo-scientific development. Firmness in dealing with children in their own interest must certainly not be confused with dominance; it is a particular instance of constructive leadership, and absolutely essential in the treatment of delinquents.

To return to the general argument, one effect of the traumatic or prolonged or repeated unpleasant situation is to impose on a whole area of the child's behavior a rigid pattern. From now on, that part of the individual's behavior will have all the properties of an instinct—crude trigger stimuli, stereotypy, and compulsive action.

But something happens at the same time to the exploratory drive. We have previously put forward the concept of *vacuum inactivity* (30), a process by which the overt behavior of an instinct system is shut off completely without the attainment of real satisfaction, by means of a central nervous process which we called a *phantasy of gratification*—a false or spurious feedback cut-out stimulus of central origin. Now exactly the same may happen in the case of the exploratory drive, whose consummatory situation is familiarization with a sector of the environment or the solution of a behavior problem. Exploratory behavior—a search for some new, more satisfactory, and undictated mode of dealing with the situation —will necessarily continue in the situation discussed unless, as actually happens here, it is shut off by means of a *phantasy* that the solution has been found, in short that the rigid adapted behavior is the right one. Thus, every such rigid patterning will be accompanied by a central computing process whereby it is falsely computed to be appropriate.[7] In other words, the behavior, however inept and forced, will be *rationalized.* The process must necessarily set up in the organized matrix of the *C* factor a body of false information (30, 52)—information, that is, which merely serves to perpetuate inadaptive behavior.

"Philosophy, according to Bradley, is the finding of bad reasons for what we believe by instinct. But most of what we believe by instinct turns out, on analysis, to be merely what we happen to have picked up in childhood" (38). We can, of course, see the process of rationalization at work, often quite superficially, in adult life, and not only among philosophers. We are all familiar with the relatively intelligent anti-Semite (anti-Negro, anti any other minority) who cannot help noticing that his behavior to Jews (etc.) is absurdly stereotyped and irrational, and therefore, since he cannot abandon it, accounts for it by some elaborate sociological theory. This explanation may just satisfy him if he is not too intelligent, and therefore it can be conscious. It is probably bolstered unconsciously by a patently mythical belief such that any Jew (Negro, etc.) is physically the same person as himself when a small, angry, and bullied child.

[7] Our present formulation has, we feel, been influenced by the related concept of a "forced answer" (that is, the solution is "given" or preset, as here, and the computing apparatus of the brain must develop a series of computations designed to arrive at it—"cook the books," as it were) which we heard put forward in an unpublished lecture in 1951 by Mr. R. Skemp (65). It is a pleasure to acknowledge our debt to him.

We may note here that the unconscious belief just mentioned is unconscious precisely because it is patently absurd. At all costs it must not be available for comparison with other data, or it will be rejected and the dangerous process of exploration will begin again. In this way, more and more false information, often of the weirdest kind, is sealed off and rendered in some way inaccessible to the mechanism of attention; this of course is what is meant by unconscious; the process of blocking attention in some particular field is repression, and the blocking of comparison of two or more sets of data, which are thus sealed off from each other, is called isolation. It is most dramatically illustrated in cases of hysterical split personality, but appears whenever we are unable to use in one context information we have acquired in another. Repression may conveniently be classified into repression of features of the contemporary environment, repression of one's current feelings, repression of past environments, and repression of past feelings, where "feelings" stands for central nervous processes to whose state, in principle, we could attend. Repression of features of a current environment which were perfectly adequately signalled from the periphery is nicely illustrated in a simple experimental situation of Halstead's (26, p. 116), where a subject exposed to anoxia while responding to silhouettes of aircraft "repeatedly denied ability to 'see' the briefly exposed form, yet . . . made perfect discriminations throughout." Despite a few such clues, no systematic attempt has been made, to our knowledge, to relate the behavioral phenomena of repression and isolation to the physiology of the cortex and the reticular formation activating mechanism which is currently supposed to underlie the process of attention (45). Here, however, remaining on the behavior plane of enquiry, we note in conclusion that the whole process, which continues ever more elaborately throughout life, serves to prevent the individual from investigating the spurious sources of his rigid behavior, for then sooner or later he would come upon that terrible group of impressions which initially switched off his exploration. Indeed, there now appears what may be called a new kind of *aversive* behavior, (18), the exact opposite of the appetitive behavior of exploration: that persistent evasion of exploration by oneself or others which is called *resistance*.

Thus, we have a sinister parody of intelligence functioning all this time to prevent re-exploration. The elaboration of phantasy may be remarkably detailed and complex, for all the mechanisms of computation in the brain are available for the purpose no less than for genuinely adaptive processes. Indeed, as Wiener seems to suggest, the pathological computing process may actually crowd out the healthy one (74).

In all of this there is a thorough-going return to *isolated* functioning and a creation of barriers to free recall and comparison of information which almost perfectly simulates, in an extremely pathological way, the instinct systems of lower vertebrates. In this weird new formation, systems of phantasy with their associated stereotyped modes of behavior take on the authority and stability of hierarchies of instincts. Between these new instincts a network of competition and mutual inhibition develops, issuing in secondary conflicts and their resulting compromises or displacements. A man may, for instance, want to go to bed with a given woman from totally irrational motives, and be inhibited by others, no less irrational. The instinct system is back with a vengeance.

Oedipus and the systems of defence

We must now notice that at the start of the whole pathogenic process several variables are of importance. On the one hand there are the four intelligence factors, doubtless varying genetically, on the other the degree of stress imposed upon the child. When we add the particular combination of neuroses in the parents and the detailed balance of their relationship, it is easily seen that a very great variety of personalities can be deduced; a basis is also provided for their classification in various ways. We cannot here pursue the question of characterology. Let us simplify by considering the stress on the one hand, and the exploratory drive (*P* factor) on the other, assuming equivalance of the other factors, with relatively high levels of *C*, *A*, and *D*. Now if the stress is small and the drive is high, inhibition of ex-

ploration will not readily occur, and the result will be an individual of considerable realism and adaptability, capable of effectively intelligent behavior even in the social context.

An interesting situation arises where a strong drive is associated with a very severe stress. The result will be a tremendous complexity and intricacy of phantasy construction. Such a person will certainly be unable ever to remove the source of his own rigid behavior, but he will not readily be satisfied by his own rationalizations, and will thus be forced to construct ever more elaborate defensive phantasy systems. Out of this conflict great literature has often emerged. The crowning example is that of Sophocles. On the face of it, his life appears to have been successful and happy—cf. the verdict of Aristophanes in the *Frogs*—but the underlying pathology emerges in his relations with his own sons, expressed in life in the law suit they brought against him, which he won by reciting a chorus from the *Oedipus Coloneus*, and in his work by the savage curses of Oedipus on his own sons in that very play. The supreme expression of his defense system appears in the play of his middle years, the *Oedipus Tyrannus*. The miraculously taut and complex construction of the play testifies to the solidarity of the defense system. If, as one of us did (W.M. S.R.), you read the play before having heard of Freud, there is no doubt what your central impression must have been, and this impression we find in the pre-Freudian editors. The moral of the play could hardly be more explicit: don't, whatever happens, explore; and above all, *don't explore the events of your own childhood*. Tiresias, Jocasta, the herdsman, all try to dissuade Oedipus, but he continues to explore, and meets with disaster, after which he puts out his own eyes, explicitly in order not to see his parents in the next world. The play is a gigantic trick, which deceived even Freud (a not dissimilar personality in some ways). For he took it at its face value—that the disastrous discovery was that of the child's lust for his mother and hatred of his father. Comforting belief! when the real terror is of recalling hostile and seductive attitudes of the parents, and one's reactions to them! We may note that the whole myth starts with Oedipus's parents fearing the little baby and trying to murder it, one using the other to this end (indeed, the fear antedated his conception, which they tried to avoid, and which only occurred because they got drunk). The lust of Jocasta for her son appears with great clarity in the finest of all modern treatments of Greek myth, Jean Cocteau's *Machine Infernale*. Far more could of course be said about the Oedipal defense, of which we have sought to explore layer upon layer; and it is already possible to trace the course of exploration and defensive construction in the work of Sophocles, Shakespeare, and others; but these topics must be left for another paper.

A CRITIQUE OF PSYCHOANALYSIS

The method of free association, discovered by Freud and forming the basis of psychoanalysis, is a device whereby isolated scraps of true and false information are brought together in the communication system consisting of patient and analyst. The analyst must continually confront the patient with his absurd and contradictory beliefs and point out discrepancies between these and the facts. In this way, by helping him to discard false explanations, and encouraging free play of exploration, the starting situation is reached and seen to be no longer traumatic or even relevant for the patient's behavior. (There is a danger in the unmodified use of this technique, which shall be considered later). But in large part the analyst's work consists in stripping away layer upon layer of phantasy. In these circumstances, a heavy strain is imposed upon his own realism, and the occupational disease of psychoanalysis has been a tendency sooner or later at a point where the exploration becomes stressful for the analysts, to believe the phantasies of their patients, and disguise them as scientific explanations. One whole school, the Jungians, is no longer scientific at all, but has become an avowed phantasy system, cherishing mythical beliefs as valuable; others are more or less tainted with phantasy thinking, an important reason why their theories have not appealed to other scientists, who have therefore paid too little attention to the actual observations. Although every *observation* made by Freud was impeccably

accurate, and although this great man to the end strove to discriminate in his papers between observation and interpretation, he succumbed, as we have seen, to a belief in the biological significance of the Oedipus complex, and even, later, developed the Gilbertian concept of the death instinct. And this fatal tendency has hampered the scientific development of the subject since his death, though considerable advances have been made in some fields. Biological and especially ethological sophistication is a great safeguard here, as Freud himself recognized in principle when he repudiated the medical and recommended a biological education for psychoanalysts (24). No biologist who attends to the matter can possibly accept the Oedipus complex as anything but a secondary distortion of behavior and system of phantasies,—though as such, like everything Freud observed, it is certainly genuine enough.

Modes of neurotic adaptation: cynicism, idealism, and the authoritarian personality

We may now return to the traumatic (or prolonged or repeated) stress situation. It is the *totality* of this whose recall is shunned, and parts of it may be explicitly or implicitly accepted when the pretence systems and modes of behavior are established, and even woven into their fabric. One line of defense, which in general we may call *cynicism*, is to accept the parent's hostile mood, and adopt it, with the corresponding behavior, in relation to other people. But the cynic shuts off from joint recollection his own feelings in response as a child; a confusion arises between his own past feelings and his own past environment (the feelings of the parents); this leads to a compulsive cruelty to others, who are treated as he himself was, by an unattractive perversion of the Golden Rule. Conversely, the *idealist* recalls and indeed perpetuates his own feelings of terror and distress, but cannot face the remembered hostility of the parent; while the cynic assumes everyone is hostile, and is eager to forestall attacks by attacking himself, the idealist is never able to admit the extent of real hostility shown in fact by other adults, and frequently acts against his own interest as a result. The two outcomes may be illustrated by countless examples in literature and history; perhaps the most vivid juxtapositions of the two types occur in Rex Warner's novel, *The Professor*, and Robert Graves's novel *Count Belisarius*, which well bring out the point that, while we may find the idealist the more sympathetic, both types of pathology are profoundly dangerous at the cultural level. An important aspect of this is that while the cynic is only too *eager* to *dominate*, the idealist is *frightened* to assume genuine *leadership*. The two types may also be illustrated by two kinds of phobia. Cynicism may be expressed in certain types of animal phobia (there are of course countless varieties of these, both in respect of stimulus object and pattern of response), in which the subject is aware of feeling in response to the (often quite harmless) animal rage, fear, and disgust, and attributes to it the wishes he himself had as a child, i.e., to approach and touch the angry, frightened, and disgusted parent, and he is forced to attack and kill the animal, acting out, in an exaggerated way, what his parent or parents in certain situations did to him, or seemed about to do by their intention movements. Conversely, in certain types of height phobia, analysis reveals a feeling of simple fear mixed with longing dating from such a situation as a wish to approach the parent when first learning to walk. But in this instance the attitude of rage and withdrawal felt and shown by the parent is felt in the phobia only as an uncontrollable urge to leap off the height and die. (Other height phobias, however, are associated with disguised hostility and a wish to throw someone else off). In concluding this account, we may note that since in adult life some of the people one meets are hostile and others are not, both cynic and idealist are unable to form accurate estimates of the attitudes of others, and suppress in this respect the operation of their intelligence. And finally, there is no doubt that every degree of mixture of the two patterns may be found in a given individual. (It should be noted that our terms "cynicism" and "idealism" are related roughly to the usage of the two words in common speech. They have nothing to do with the terms applied to ancient or modern schools of philosophers. Plato, for instance, was certainly a cynic in our sense).

The idealist type of personality demands much more study than it has yet received. But one extremely important negative point can be made, by the further analysis of cynicism. We have seen that this always involves a preoccupation with attaining dominance over other people; indeed, since the cynic is confused about his own wishes (these at least are clear to the idealist, though he may be unable to satisfy them), his *only* goal may be domination. Now dominance may be sought in two ways, to which correspond what we may call *overt cynicism*, or *sadism*, and *covert cynicism*, or *masochism*. (Needless to say, we again use two polar extremes for convenient description of what is in fact a spectrum). The overt cynic, though of course deceiving others, consciously and deliberately pursues his goal; the covert cynic deceives even himself, by the addition of a further layer of phantasy. These two types may be specially clearly illustrated by a comparsion of Molière's *Tartuffe* with Dostoevski's *Friend of the Family*, since the second follows the first closely but with a different psychological slant. The covert cynic, or masochist, is seen in the extremely familiar type of the "martyrs" who dominate others by suffering themselves, obtaining attention by illness and the like. The method is insidious, since it may not even be recognized as a mode of dominance, especially by idealists. (This comes out clearly in the film version of Steinbeck's *East of Eden*). We may, therefore, distinguish open dominance from dominance-through-apparent-submission. Now the presence of the additional layer makes the masochist superficially similar to the idealist, and the two are all too readily confused.[8] Fortunately the three personality systems can be distinguished by certain indicators, namely *triumph*, *guilt*, and *shame*. Triumph, a pleasurable feedback awareness of having successfully dominated, is found in the sadist; guilt, which is simply triumph disguised in order to ensure that dominance behavior can continue to be indulged without being recognized by the guilty person or others for what it is, is associated with the masochist; guilt is never found in the idealist, who feels only shame, a disguised awareness of having been ill-treated and of having submitted.

The three modes of neurosis may best and most clearly be studied in *Othello*. (We may remark here that while it is indeed proper to use fictional characters for the analysis of the *author*—as in the imaginative studies of Ella Freeman Sharpe [64]—it is also possible, where an author is a superb observer, to use his characters as models for actual types of individual. Instead of listing masses of clinical observation, one can direct observation more succinctly by such references. It is easy enough once the model has been studied, to find individuals in real life who are—admittedly less heroic—versions of Hamlet, Cleopatra, Coriolanus, etc.) We must, of course, always remember that in this section we are considering *modes of behavior* rather than *types of individual*; nevertheless some individuals restrict their behavior so completely to one or other mode that they approximate to types.

We may, then, see in Iago the pure overt cynic, or sadist. He is throughout, totally free of the slightest guilt, or, when he is finally caught, shame; his only response to the situation he has brought about is one of overt triumph. He is completely cynical about other people, always ascribing the most unpleasant motives possible. His overriding goal, as Bradley (12) saw long ago, is domination over others. He is, indeed, not perfectly pure: there remains, as Bradley also noted, some need to rationalize his behavior to himself in his frequent soliloquies—a masochistic trait.

In Desdemona we see an unusually clear-cut portrait of a true idealist, her only mistake being her failure to realize, either before or after the marriage, the dangerous underlying hostility of her husband. She is, characteristically of this type, totally free of guilt, e.g., about her elopement. She does, however, suffer from shame, a refusal to face the cruel way in which she is being treated. Sooner than face and investigate Othello's outrageous behavior about the handkerchief, she resorts to lying and puts herself in the wrong (the idealist will always prefer to recognize, correctly or otherwise, something

[8] For an excellent example of such a *pseudo*-idealist, cf. *Timon of Athens*.

wrong with his own behavior than something wrong with that of others). Finally and spectacularly, she lies even about the murder as she is dying, as though even then she cannot bring herself to admit the destructiveness of her husband.

Finally, in Othello, we have a superbly subtle and expressive study of the covert cynic, or masochist, and the play mainly turns on a brilliant exposure in which the secondary layer is stripped away to reveal the underlying cynicism and sadism, culminating in the murder. After this, he reverts to a masochistic pattern in a speech full of guilt, patently designed to deceive himself and the others about the true nature of what he has done and what he is, and to distract their attention while he resorts to the usual response of the thwarted sadist—suicide. Our triple classification is expounded with the most utter clarity in this great play.

An interesting mode of expression of cynicism is seen in the now well-studied *authoritarian syndrome* (1), in which both sadistic and masochistic behavior is employed, but in response to different objects. (In view of the term which has been used, it is important to note that this syndrome is only one way of expressing a preoccupation with dominance, and that it is characteristic of people in the middle of a dominance social hierarchy, and not of dictators themselves.) This usually results from a family with one overtly submissive and one extremely openly dominating and threatening parent, and in which the child is often thwarted and in particular specifically punished for the slightest expression of rage against his parents. As a result, the extreme authoritarian, when adult, divides the world into two categories, marked by key stimuli as simple and crude as any found in lower animals (apart from a certain degree of abstraction): to one group he cringes, while the other group he persecutes. Actually, he felt rage and fear in response to the same person, so now he splits the world into clean, powerful, "good" adults, whom he must appease and fawn upon, and dirty, weak and "bad" children (typified by racial or other minorities), whom it is safe and legitimate to attack. Under the influence of this syndrome, which is involved in, for instance, race persecution, an adult who can show considerable intelligence in nonsocial situations may be reduced in his dealings with other humans to the level of a lamprey. His capacity for discriminating between individuals has dwindled to an absurd rule of thumb, his social behavior to two simple alternative stereotypes; he tolerates the most ludicrous inconsistencies (e.g., *both* that members of a hated minority are too exclusive *and* that they merge too much with the rest of the population) and thinks in water-tight compartments; he shows a remarkable degree of *isolation*, and an extreme rigidity (no information which would correct his views is ever attended to at all). These points all emerge with great clarity in the brilliant study of Sanford and his colleagues (1). We are all, of course, authoritarian in some degree (it is a quantitative variable), and this well-studied illness strikingly illustrates our thesis that the factors we are discussing render behavior more instinctive.

It is of great interest as a matter of interspecific relations that animals, which often stand to man in much the position of helpless children, readily become candidates for the out-group (the set of persecutable objects). It has been shown that certain pathological attitudes to lower animals are significantly correlated with the authoritarian syndrome, and also, interestingly, that the latter is correlated with a stereotyped preference for dogs over cats, clearly related to Lorenz's observation that dogs, unlike the nonsocial cats, react to man in terms of a dominance hierarchy (29, 59).

Before concluding this section, we should dispel any impression of oversimplification. Naturally any comprehensive human characterology will have to employ many coordinates. The position of a given individual on, for instance, the authoritarian scale, gives us only one of these, and is certainly not a complete description of his personality.

Nevertheless, to complete the particular classification we have considered in this section, we must point out that the idealist will appear at the "liberal" end of the authoritarian scale (though the two terms may not be quite coextensive). The sadist and masochist will both score high, but will be discriminated by their response to two

subscales distinguished by the authors concerned (authoritarian aggressive and authoritarian submissive, see Ref. 1).

The classification has an important bearing upon psychotherapy. In treating the cynic, it is extremely important to induce him to examine his own motives and feelings, and to point out when he is ascribing his own hostility to other people. Orthodox Freudian psychoanalysis usually takes this course, but is open to the serious danger that it may simply turn masochists into sadists by providing them with unusually successful *rationalizations*, if their phantasies are at any point treated as *explanations* of their behavior. But in any case this method is quite unsuited to the treatment of idealists, who are only too ready to ascribe their troubles to their own difficulties, and must at all costs be made to face and recognize the hostility of others when it actually exists. The process of therapy must therefore be a carefully balanced and timed one, varying with each patient, of inducing exploration of his own moods and of those of others (the latter may be assisted by frank communication of the analyst's own reactions—a course which, of course, is fatal in dealing with cynics).

Although these principles can be shown to have some success in practice, the problem of the idealist is much further from solution even than that of the cynic, with whom in effect psychoanalysis has chiefly been concerned. It is a vitally important one for the species as a whole. Extreme cynics are somewhat hopeless cases by all existing methods, since their repression of their own original feelings is associated with a radical distortion of their goals into destructive ones. The idealist may at least have relatively constructive goals. But his contribution to the progress of man has been seriously limited by his unbounded capacity for being deceived, utilized, and murdered by the cynic. We mentioned previously that every political revolution, starting with a genuinely new social idea, becomes almost immediately a power struggle (between cynics). The turning-point is regularly marked by a slaughter of idealists. The process has been caught and pinned down on paper, with exquisite concision, in Orwell's famous parable, *Animal Farm*. Again and again in studying human history we watch successive groups of idealists—Theramenes in Athens, Cicero in Rome, the Girondins in France, the Mensheviks in Russia—plodding, with what seems in retrospect astounding blindness, towards the hemlock or the guillotine. One is continually tempted to exclaim, like the legendary play-goer, "Look out! He's going to murder you!" If the cynics have contributed countless crimes, the idealists have imparted innumerable follies to human history. It is of extreme urgency to understand their particular neurosis.

Finally, we must stress that in using these terms we are clearly concerned with cynicism or idealism about other people's *hostility*. The words, in common usage, are sometimes used with a different meaning. Thus fanatics are sometimes called "idealists," but are invariably cynics; similarly, to the Puritan all things are impure. A final example will show that our sense of idealism is not incompatible with sexual aberration or with a superficial flippancy readily mistaken for cynicism, and it will also well illustrate the astonishing risks which the idealist is led to take through his stubborn refusal to face the hostility and cruelty of others. This final example is the extreme idealist Oscar Wilde. His libel suit against the Marquess of Queensberry, often cited as a case of quite inexplicable folly, was clearly the result of his failure to face this kind of fact. He plainly *could* not believe that a barbarous and sadistic law would ever be invoked against him, and his final conviction and imprisonment came as a complete surprise.

Systems of phantasy

The literature of Freud and his successors is full of an extraordinary welter of bizarre and even ludicrous phantasies about bodily parts and processes, together with the many secondary symbols of these first discussed by Freud himself in his great book *The Interpretation of Dreams* (23). (We may mention in passing that the process of symbolization may often be accounted for by a grouping together of objects or processes *to which the parents reacted with similar moods*.) To borrow a useful mode of analysis from Lorenz (50), there are two reciprocal errors in this connection. One is to discard all Freud's

observations and dismiss as trivial or false the occurrence of the phantasies themselves. This course, in greater or less degree, was followed by many of the deviant "schools," such as that of Adler. The other is to treat these phantasies as in some mysterious sense biologically basic, to be used as principles of explanation or elements in a theory. This course, common especially in the Kleinian school, tends, naturally enough, to make scientists sceptical.

Now there is absolutely no doubt that phantasies about breasts, penises, feces and so forth are to be observed with extreme frequency by anyone employing psychoanalytic methods, and may in any case be traced, more or less disguised, throughout human literature and other arts. It is, however, crucially important to note that when their analysis is ruthlessly pursued we always come, sooner or later, to phantasies and ultimately direct reactions to *social situations and relationships* (initially in the family).

We cannot possibly illustrate this generalization in detail in the present paper. One or two summarizing illustrations of the principle may, however, be given.

Thus we do indeed find the following system of phantasies in some males: (*a*) that the penis is of immense importance and castration a fate worse than death. There might seem to be some sense in this notion, until we realize that it is quite unrelated to the actual function of the penis, and that a man with this phantasy may never have intercourse at all, for fear his penis is destroyed by (*b*) a set of teeth in the vagina (one of the many remarkable beliefs about this organ). Further analysis sheds a little light, when we find that (*c*) the penis is believed to be a *breast*, and (*d*) semen to be milk, for it is then not altogether surprising that the vagina should be (*e*) confused with a mouth.

A patient will go on elaborating this sort of nonsense with *relatively* little resistance, but on probing further we discover the reason, namely, it is much less alarming to talk about breasts and penises than to consider the *social* and *emotional* implications of all this. In fact, this type of phantasy is itself a disguise for a belief of the patient in his own self-sufficiency, his utter lack of any need for social relationships of any kind, even in such a context as sex. This belief in turn arose as a result of the unpleasantness of situations in infancy when the assistance of another person was indeed necessary for almost all kinds of gratification, and the behavior of the other person was plainly quite outside the infant's control, and not smoothly or regularly related to his needs and wishes. The patient and his penis now reflect in phantasy a social relationship between two separate individuals, one of whom is completely under the control of the other (cf. the personification of the penis in literature—especially common in the *Thousand and One Nights*, whose heroes are constantly talking to and having "social" relations with their penises). Castration will now come to symbolize complete desertion by the mother. The pseudo-social situation is soon complicated when the original state of affairs is made to reappear in, e.g., the phenomenon of impotence—the penis is no longer under control. If another actual individual is woven into the phantasy system, more troubles arise; the patient wishes, in this set-up, to play the role of the person vis-à-vis whom he was helpless and dependent (i.e., the mother), and now becomes confused about his own original mood and that of his new partner—he expects the latter to attack him, because he originally, as an infant, felt considerable rage when in the frustration situation. Hence the teeth. Innumerable variations on this sort of theme may be found, and individuals and parts of the body are constantly being interchanged in phantasy. More fundamentally, the confusion is always between information about the various parties in the past and present situation, and specifically about their moods and social reactions. Always, when we explore far enough, we come to situations vis-à-vis parents such as we have discussed in this paper, ultimately stemming from some defect or distortion of the parents' parental behavior and the infant's reaction to this. (In the present instance, this sort of phantasy arises only if the mother's behavior in the feeding situation was more or less grossly unrelated to the infant's needs, e.g.,

if it was based on a fixed schedule of intervals between feeds.)

Another of these curious "body" phantasies is the belief that the penis is a dangerous weapon by means of which others can be dominated or damaged. This goes back in one direction to a situation in which the child himself was used as a pawn in some dominance game between the two parents, and issues in the other direction in social behavior in adult life in which a woman (now representing the penis, which represented the child) is used as a means to power over other males. This results in a particularly vicious form of jealousy, for at all costs the weapon must not fall into other hands, and also clearly becomes woven into homosexual phantasies, relations with other males being arranged at second hand through the woman. Here a set of phantasies ostensibly about penises can soon be traced back to a dominance social situation.

This last instance also illustrates an important aspect of pathology—the mutual linkage of initially separate drives and activities. This coupling of drives is partly the result of a mechanism of perpetuated displacement postulated by one of us (C. R.) and put forward elsewhere (30), and perhaps even more the result of combinations of reactions on the parent's side. All drives, in particular, come to be coupled to the sex drive, so that in sexual activity a variety of other drives come into play, which must be neutralized by various combinations of overt behavior (perversion) or phantasy (perverse phantasies—for the mechanism of acute or immediate phantasy, cf. Hayes *et al.* [30]), if sexual activity is to be indulged in at all.[9] In particular, sexual relationships are liable to be inextricably entangled with dominance struggles—a theme in which literature abounds but which has perhaps been most explicitly developed in the novels of Stendhal. It is evident that a vicious cycle can thus develop through the generations, for this sort of relationship between parents is the starting-point for the whole process.

[9] For this last analysis we are indebted especially to J. S. Hayes, who has developed a similar analysis for obsessional symptoms, where the drive concerned is aggression (28).

Transference and identification

We have considered the detailed relations between the behavior of parents and offspring, and mention must be made of the two mechanisms called by psychoanalysts transference and identification. Here again we must be careful not to confuse phantasy with explanation. It is usually said that a patient falsely believes that some individual (e.g., the analyst) is his mother or father (transference), or that he believes himself to be one of his parents. This may be an accurate observation, but we hope we have made it clear by now that phantasies are only *rationalizations* for patterns of behavior preset in some way. The basic observation is that the patient has been forced by the detailed nature of his parents' behavior, and in response to their conscious and unconscious designs upon him, to adopt a particular mode of behavior, which may be highly detailed in patterning, and which is quite rigid and unmodifiable by later experience. He may indeed then rationalize the behavior by a belief of his identity with one or other parent. In this sense he will "identify" with the parent he used as a model in dealing with the other. Usually, the resulting personality will be a mosaic pattern, made up of "identifications" with each of two parents in a number of different contexts, together with the reverse process in other situations—extreme and equally rigid reversal of the pattern forced upon him. Each individual is thus, insofar as he is neurotic, a sort of compromise or resultant of the personalities of his parents. At this point an interesting corollary arises. If the relationship between the parents is extremely stable in a neurotic sense, and they form, as it were, a united front, it may be relatively easy for the child to adopt a behavior pattern which satisfies both. When he grows up, such an individual has an extremely rigid personality. If it happens to accord with the rigidities of his culture or society, he will be considered "normal," but of course his personality may none the less be highly pathological, as will readily appear if he is put in another environment. If, on the other hand, there is considerable conflict between the two parents in many fields, the child may be unable to

form a "satisfactory" compromise, and this may mean that he *goes on exploring*. Such an individual is likely to be considered "neurotic" when he grows up, and will certainly display symptoms of "anxiety," but he will more readily respond to analysis and he may be much more capable of creative activities, in general of exploration, than the stable conformist. A similar result may arise from a child's exposure to several different social environments. This phenomenon may well have saved mankind from the worst consequences of the whole pathological system we are discussing, since it permits some variance to enter upon the scene.

It may be noted that perhaps the most pernicious example of taking phantasy for explanation has been the notion in psychoanalysis that one particular type of identification (e.g., of a boy with his father) is desirable, or at least necessary. It is obvious that the sort of compulsive and secondarily rationalized process we are considering is wholly pathological, and that deliberate, rational selection of certain behavior of a parent as a model, to be verified and discarded if later found inappropriate, is quite another matter.

Once a pattern of behavior is adopted in the rigid way, the individual, in his dealings with others, has only three alternatives. Either he rationalizes his social behavior by ascribing to his "friends," sexual partners etc., attitudes and moods they do not in fact show (transference), or he does it by unconsciously choosing for his more long-term relationships individuals who actually behave in a complementary way. Or, most pernicious of all, he uses every artifice of dominance to *make* his associates behave thus. His children are, of course, at the mercy of this molding process, and once again we see the cycle repeating itself.

Types of transformation. The role of suggestion

It is all too clear to us that most of what we have said in this paper is essentially descriptive. We believe it is, indeed, extremely important to show to how great an extent psychoanalytic theory to date is merely descriptive, and how parts of it have been mistakenly treated as causal explanations. Systematic description is indeed valuable, provided its nature is not misinterpreted. However, we have advanced some hypotheses, if of an extremely general kind, and we have almost inevitably simplified in earlier sections a situation which is plainly exceedingly complex, involving the interaction of an astronomic number of causal variables. In this section, therefore, we shall try, at a purely descriptive level, to broaden the basis of our discussion, and give some indication of the immense amount of unexplored problems in even the pathology of human behavior.

The last section inevitably leads to a consideration of the way in which human personalities may be *transformed*. In this connection we may make a thought-provoking analogy with the brilliant classification introduced some years ago by Medawar in the quite different, but in some ways formally analogous field of cellular transformations. We do not intend to push this too far, and shall not here summarize at length the argument of his paper, but may usefully consider some of its features (54).

Adaptive transformations may be illustrated by the formation of antibodies and by the development of new enzyme systems in bacteria. The characteristics that here interest us are: the specificity of the transformation is determined by the stimulus situation; the transformation is initially reversible, but becomes less the longer the exposure to the stimulus. Now reversible transformations undoubtedly occur in the present context. The whole behavior of an individual may change—apparently his personality—if he is placed for a time in a particular cultural environment, e.g., in a police state. But, at least in some cases, he may revert to a totally different behavior on being restored to the original, or placed in a third social situation. We may instance the chameleon-like behavior of Alcibiades during his sojourns in the various states of Greece and in Persia. It is a tenable hypothesis that the sort of adaptation to the parental situation we have just discussed may be simply of this form. The rigidity of the personality so produced might be the result of the length and intensity of the exposure (a concept which of course would require further analy-

sis). The fact that the behavior of *some* individuals in *some* ways does become more adapt*ive* (i.e., less adapt*ed*) in later life, whether or not as a result of psychoanalytic treatment, points at this sort of interpretation. But we are at once faced with the general question, how far is the effect irreversible and what, fundamentally, makes it so? In other words, the problem of rigidity or of instinct itself.

Without following Medawar's analysis in detail (it will not fit perfectly, being designed for a field with certain inherent differences from ours), we may consider some further points raised in his discussion. One is the apportionment of specificity between the stimulus situation and the responding cell (in our case, the responding individual). We have tended thus far to assume that the specificity of the effect was wholly determined by the stimulus situation, i.e., that the individual's personality can be related in the finest detail, point for point, to the total stimulus situation made up of the personalities of his parents and other individuals to whom he was exposed in childhood. But this is an assumption; moreover, we admitted earlier the obvious plausibility of relevant genetic differences between individuals. The problem is one that will have to be pursued eventually. It can, of course, in principle, be phrased quite precisely in terms of relative amounts of information, in the technical sense, or in terms of path coefficients (75). What we hope to have made clear is the *relatively* enormous contribution of the environment to specificity in this particular aspect of the human phenotype.

It is, however, interesting that we can find one clear instance here of what Medawar calls simple *activation*, that is, where the specificity of the change is wholly determined by the intrinsic properties of the responding system, the exact nature of the change is predetermined and unique, and the change itself can only occur in response to the stimulus. Battle situations of extreme stressfulness have been shown to produce two effects (42). The first is the war neurosis proper. This we shall not discuss here, beyond mentioning that its analysis by Kardiner and his associates has introduced a whole new set of fundamental mechanisms, which any complete theory of human behavior must take into account; the reference may serve to emphasize the restricted nature of our present synthesis. The second effect is an *intensification* of whatever particular neurosis was present in slight or latent form before the stress was imposed, and unlike the war neurosis proper this effect is specific to the exposed individual.

Finally, we may consider the bearing here of one particular characteristic of what Medawar calls infective transformations, namely that "the presence of the transforming agent or a replica of it is a necessary condition for the maintenance of the transformed state." Now in a sense this is obviously true for the pathogenic processes we have been considering, for replicas of the parents' behavior control specifications do indeed come to be set up in the child. But so far we have considered this as coming about indirectly as a result of the child's own adaptation to a situation which severely limits his choice of alternative modes of behavior. May there not be a direct mode of infection?

This brings us at once to a vitally important and all too little studied problem, which we have so far ignored, namely the role of *verbal suggestion*, a process whereby a complex, highly organized control device is passed from one individual to another, like a virus, with only the simplest coding and decoding en route. If, as seems likely, speech at its first appearance was intimately related to dominance the possibility is inherently great. But we need not rely simply on deduction. The astonishing potentialities of suggestion, for good or ill, are already clear in works written decades ago (8, 13). Perhaps the most urgent problem in this context is the detection of the factors making for suggestibility, the conditions under which verbal inputs, instead of being analyzed and then integrated with other information, are accepted and retained with the status of instincts. But the whole field of suggestion is one urgently needing exploration, not simply as a possible ingredient of pathology but as a potent technique for advance.

It is perhaps important at this point to note one instance in which verbal suggestion is almost certainly more important than

more indirect modes of adaptation. This is the process, habitually observed clinically of "identification" of the patient with the analyst. Since this occurs most readily when classical orthodox techniques are employed —that is, when the analyst aims at minimizing any trace of social relationship with his patient—it seems clear that this type of transformation (the so-called transference neurosis) is of a special category. The process of free association in extreme form must lead the patient to weaken his own ability to *attend* to and organize his feelings—a role all too liable to devolve upon the analyst, whose verbal interpretations may come to be the patient's sole principle of organization. This danger is of the utmost moment in the clinical context, and the greatest vigilance must be employed to avert it; in particular, it becomes more and more necessary to train the patient to use his own attention, and to concentrate upon particular problems on his own initiative.

Finally, we may relate the role of verbal transfers to our discussion of stereotypy and rationalization by noting that the performance of posthypnotic suggestions is always *rationalized* (13), often by the most farfetched excuses, which thus disguise from the subject the compulsiveness of his action.

We must note now that throughout this paper we have virtually ignored another extremely important factor—the autonomic nervous system. It is virtually certain that the stress situations whose results were considered above must be accompanied by characteristic autonomic discharges, probably often by mass sympathetic responses, with accompanying endocrine effects. The autonomic response will doubtless also fail to be explored (autonomic processes are notoriously unamenable to conscious inspection and control, as their name implies, despite anatomical evidence for cortical connections). It is at least a suggestive hypothesis that this uncontrolled, unobserved, uncompensated, and physiologically drastic type of response may be connected with the rigidity of the resulting conditioned instinct formation, though it would be premature to reduce this conjecture to detailed cytological terms. It may therefore be of great therapeutic importance to develop the capacity to inspect one's own autonomic activities which may otherwise not inconceivably play a part in the process of resistance.

The autonomic and endocrine systems are obviously crucial also for psychosomatic disorders, but with this problem we are not concerned in the present paper, which deals only with the causation of somatic behavior.

In this section we have raised a number of questions, with no attempt at finding ready-made answers. While we hope the synthesis we have presented will ultimately help in the integration of these important problems, we make no claim to completeness, and hope this account will set our endeavor in perspective.

THE CULTURAL LEVEL: ART, SCIENCE, AND SOCIETY

We must now consider the bearing of our analysis of individual behavior upon that of whole societies and of the whole species. We may begin by examining the divergence of the two great positive, progressive, and creative human activities, *art* and *science*. The divergence, which is by no means necessary or inherent, stems in our view from the block imposed in the situations discussed. Although the two activities are felt as almost identical by the individual performing them —a fact for which we can personally vouch— and although both consist essentially of exploring and communicating, there are undoubtedly at present marked differences between them in practice, and a social stereotype has grown up to this effect, issuing in such ridiculous disputes as that about the relative importance of science and "the humanities" in education. We may list some of these differences. First, science has developed a reliable technique of social verification, the operational method, or repetition of described observation, which is virtually lacking in the arts, as appears from the complete absence of satisfactory criteria for reliable assessment of works of art. It is on account of this defect that the obvious "progress" of science is not paralleled by any such obvious progress in art. But on the other hand science has, as we said earlier, restricted its effective success largely to the analysis of variables in the nonhuman environment— the gap between the nonsocial and the social

sciences is vast. Little attempt has yet been made by the scientist to explore and communicate patterns of feeling or central nervous nuances, and some even now maintain that "conscious" phenomena are outside the scope of science. The artist, however, has been chiefly preoccupied with precisely this problem, and has often met the challenge with considerable success; a success which is severely restricted by the absence of operational methods. Now there is no inherent reason why anything whatsoever should not be made the object of operational communication (telling other people what to do in order to see what you saw). At present a paper on, say, the synthesis of an organic compound communicates nothing whatever to a large majority of human readers. Fortunately, there are a number of trained chemists, who on reading the paper can repeat the operations of the synthesis. But there are vanishingly few individuals trained to observe and report upon their own feelings. Not until such training has begun can we hope to apply in practice what is perfectly imaginable in principle—operational instructions in the field of human feelings. Thus we might say: eat a meal composed in such and such a way, go for a walk of a certain distance in a certain type of scenery, listen to this movement of that symphony in this recording on a high fidelity set of certain characteristics, free associate for so many minutes on a certain subject, give yourself this or that suggestion, and you will observe in yourself the mood, or state of mind I am trying to convey. But all this is clearly for the future. We may note one further difference between science and art as currently practiced—the degrees of freedom of the *imagination* are higher in art. But this last difference is beginning to dwindle in importance now that many leading scientists, especially in the United States, convey their more daring and speculative hunches in the new art form of science fiction.

The two main differences we have considered stem clearly from the fact that the neurotic taboo lies upon the exploration of human feelings and social interactions. It is therefore natural that artists have attempted this subject-matter while failing to refine adequately their method of exploration at the social level, while scientists, equipped with a splendid social instrument of assessment, and thus relatively free from the phantasy (in the neurotic sense) which constantly intrudes upon the artist and blurs his perception or execution, have largely shirked the problems of this field, or lost their own integrity in making the attempt. We must note indeed that before the development of science itself, even in the study of the nonhuman environment, the savage saw even this in terms of his infantile experience; his only response to unsatisfactory physical conditions was to personify them and then to bully or appease them, as if he was still in a dominance relation with celestial parents. Often the personification was dramatized with the help of human actors—priest-kings, rainmakers, and the like. The phantasy persists even in our day in the odd and curiously inept formulation that scientists are engaged in the "conquest of nature," and in other similar clichés—fossilized phantasies, as it were—such as the verbal habit of "sacrificing" experimental animals.

All this adds up, as we said earlier, to a block upon one whole vitally important area of scientific exploration; or to a steady resistance to progress in this field. It is clear that our own main contention may be summed up in the conjecture that this is due to the "dead hand" of the past—the constant molding of one generation by the next, from which escape is only sporadic and limited. There are, however, marked differences in this respect between particular cultures; it is, for instance, a truism that the European cultures and their offshoots in North America and elsewhere have developed the physical sciences at least to a degree which differs from the progress of the savage cultures as dramatically as does human behavior from that of the apes. "Savage" cultures are sometimes called primitive; the word is of course a complete misnomer, for they are, to the biologist, markedly *specialized*. The path of human progress is herring-boned with these blind alleys, as is the path of vertebrate evolution. The civilized cultures owe their continued scientific progress to an absence of social specialization. The "savage" society may aptly be called an *instinct culture*. It is characterized by a certain stabilization

at the social level of a particular set of phantasies (myths) and a particular set of rigid modes of behavior (customs and rituals). For instance, many of the great varieties of sexual modes found in a civilized culture in certain individuals may be found in the whole populations of particular savage cultures (cf. 22, 43). While no neurosis is adaptive, we have been saved by our retention of a great *variety* of neuroses, and consequently by a degree of variance within and between individuals upon which alone science and art can continue to advance. When every individual has the same phantasies and the same neurotic adaptation, art decays and science dies. But the savage human cultures are not, like the specialized animal species, irretrievably committed (and hence inevitably lost at the first change in the environment). For it is still possible for the civilized cultures, impinging upon them, to shake them from their age-old rigor.

It is interesting at this point that Ashby has shown in a highly general way (3, 5) that in any system of interacting units the *unstable* ones acquire control of the system's behavior. This is eminently well illustrated by the relations between civilized (unstable) and savage (stable) cultures.

Stability of a certain kind is of course necessary for civilization, if only to prevent lapse into one or another specialized rut, just as continuity is necessary in the development of the *C* factor in the individual. But it must be stability of a different order, or on a different level. The problem of the scientific society is the development of social institutions which can oppose any permanent rigid specialization, while permitting maximal variance of behavior. Since this is precisely the problem which in history has been attempted by liberal politicians, who have tended to suffer from the idealist neurosis, individual and social pathology are intimately related at this point. But the justification of both stable personality and stable society is directly related to their success in maintaining heterogeneity, and hence progress.

Homogeneity is thus a danger no less fatal, if less immediate than atomic war. It is therefore alarming to note the dangerous processes of homogenization at work in modern societies, their danger increased many times by the mass media of communication—television, etc.—provided by modern technology; the parallel with the development of devastating weapons is obvious. Incidentally, we note that any theory of human behavior based wholly on principles of conditioning and the deification of Pavlov is at once suspect; for conditioning is a means for destroying intelligence. The problem of resisting homogenization is urgent. D. Morris has suggested to us that one potent device may be the creation of a large number of foci of "loyalty" or cultural pressures, as in the older universities: here there is safety in the very number of competing mores and the result may be (as for instance at Oxford) virtually complete freedom from any one, and a considerable range of variance in ideas and action. We might perhaps support this idea by noting that the reforms of Cleisthenes took exactly this course, and tentatively ascribing to the work of this political genius the astonishing civilization of fifth-century Athens.[10]

Whatever the utility of this particular method, there is no doubt that the most urgent problem of sociology is the conservation of variance. If the therapist is charged with the responsibility for maintaining exploration in the individual, the sociologist has the no less grave charge of maintaining exploration in the society, and indeed in the human species.

CONCLUSION

While we fully recognize the limitations of our present synthesis, we hope it is of value as a system of relatively simple concepts, built up step by step from a very few of great generality (instinct, dominance, etc.), in terms of which it is possible to discuss in a coherent and related way problems of lower animal ethology, the social relations in the human family, the development of human behavior, the varied clinical exhibits in the gallery of psychopathology—phantasies of every kind, neurosis proper and delinquency, perversion and obsession, and so forth—the concepts, methods, and value of psychoanalysis, the varieties of human culture, the

[10] Note the parallel with the development of the individual, page 192.

achievements of art and the progress of science. Much of our argument may be wrong, but we hope at least to have formed the rudiments of a language in which, for instance, the ornithologist and the literary critic may talk to one another.

More important, we hope this paper will stimulate research in many directions where it is urgently needed. It is perhaps proper to conclude with a request to lower animal ethologists. If one thing is evident from what we have written, it is the need for systematic work on reactions of lower animal parents to their young of a kind other than parental. The lower primates may be of crucial importance, but work on any species may provide important clues missing from our brief account, and further field studies like those of Hinde and Tinbergen are badly needed.

And finally, if the picture we have drawn of human pathology seems forbidding, nevertheless its contemplation is both a spur to further effort and a promise of great reward. For if man has achieved so much with this burden on his back, what can he not achieve if these factors can be controlled and the burden removed? Then indeed the prospect is one of limitless potential. It is the supreme task of behavioral science to hasten the time when these godlike potentialities no longer "fust in us unused."

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The word elephant calls up a certain association of mental impressions, but it is clear that mental impressions as such cannot be the subject handled in the physical problem. We have, for example, an impression of bulkiness. To this there is presumably some direct counterpart in the external world, but that counterpart must be of a nature beyond our apprehension, and science can make nothing of it. Bulkiness enters into exact science by yet another substitution; we replace it by a series of readings of a pair of calipers. Similarly the greyish black appearance in our mental impression is replaced in exact science by the readings of a photometer for various wave-lengths of light. And so on until all the characteristics of the elephant are exhausted and it has become reduced to a schedule of measures. There is always the triple correspondence—

(a) a mental image, which is in our minds and not in the external world;

(b) some kind of counterpart in the external world, which is of inscrutable nature;

(c) a set of pointer readings, which exact science can study and connect with other pointer readings.

And so we have our schedule of pointer readings. And if you still think that this substitution has taken away all reality from the problem, I am not sorry that you should have a foretaste of the difficulty in store for those who hold that exact science is all-sufficient for the description of the universe and that there is nothing in our experience which cannot be brought within its scope.

—A. S. EDDINGTON in *The Nature of the Physical World*

THE CONCEPT OF POWER

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What is "power"? Most people have an intuitive notion of what it means. But scientists have not yet formulated a statement of the concept of power that is rigorous enough to be of use in the systematic study of this important social phenomenon. Power is here defined in terms of a relation between people, and is expressed in simple symbolic notation. From this definition is developed a statement of power comparability, or the relative degree of power held by two or more persons. With these concepts it is possible for example, to rank members of the United States Senate according to their "power" over legislation on foreign policy and on tax and fiscal policy.

THAT some people have more power than others is one of the most palpable facts of human existence. Because of this, the concept of power is as ancient and ubiquitous as any that social theory can boast. If these assertions needed any documentation, one could set up an endless parade of great names from Plato and Aristotle through Machiavelli and Hobbes to Pareto and Weber to demonstrate that a large number of seminal social theorists have devoted a good deal of attention to power and the phenomena associated with it. Doubtless it would be easy to show, too, how the word and its synonyms are everywhere embedded in the language of civilized peoples, often in subtly different ways: power, influence, control, pouvoir, puissance, Macht, Herrschaft, Gewalt, imperium, potestas, auctoritas, potentia, etc.

I shall spare the reader the fruits and myself the labor of such a demonstration. Reflecting on the appeal to authority that might be made does, however, arouse two suspicions: First (following the axiom that where there is smoke there is fire), if so many people at so many different times have felt the need to attach the label power, or something like it, to some Thing they believe they have observed, one is tempted to suppose that the Thing must exist; and not only exist, but exist in a form capable of being studied more or less systematically. The second and more cynical suspicion is that a Thing to which people attach many labels with subtly or grossly different meanings in many different cultures and times is probably not a Thing at all but many Things; there are students of the subject, although I do not recall any who have had the temerity to say so in print, who think that because of this the whole study of "power" is a bottomless swamp.

Paradoxical as it may sound, it is probably too early to know whether these critics are right. For, curiously enough, the systematic study of power is very recent, precisely because it is only lately that serious attempts have been made to formulate the concept rigorously enough for systematic study.[1] If we take as our criterion for the efficiency of a scientific concept its usability in a theoretical system that possesses a high degree

[1] By demonstrating the importance of concepts such as power and influence, particularly in political analysis, and by insisting upon rigorous conceptual clarity, Harold Lasswell has had a seminal influence. Cf. especially Reference 3. A similar approach will be found in References 6, 7, 8, 10. For the approach of the present article I owe a particularly heavy debt to March, with whom I had countless profitable discussions during a year we both spent as fellows at the Center for Advanced Study in the Behavioral Sciences. I have drawn freely not only on our joint work but on his own published and unpublished writings on the

of systematic and empirical import, then we simply cannot say whether rigorous definitions of the concept of power are likely to be useful in theoretical systems with a relatively large pay-off in the hard coin of scientific understanding. The evidence is not yet in.

I think it can be shown, however, that to define the concept "power" in a way that seems to catch the central intuitively understood meaning of the word must inevitably result in a formal definition that is not easy to apply in concrete research problems; and therefore, operational equivalents of the formal definition, designed to meet the needs of a particular research problem, are likely to diverge from one another in important ways. Thus we are not likely to produce—certainly not for some considerable time to come—anything like a single, consistent, coherent "Theory of Power." We are much more likely to produce a variety of theories of limited scope, each of which employs some definition of power that is useful in the context of the particular piece of research or theory but different in important respects from the definitions of other studies. Thus we may never get through the swamp. But it looks as if we might someday get around it.

With this in mind, I propose first to essay a formal definition of power that will, I hope, catch something of one's intuitive notions as to what the Thing is. By "formal" I mean that the definition will presuppose the existence of observations of a kind that may not always or even frequently be possible. Second, I should like to indicate how operational definitions have been or might be modelled on the formal one for some specific purposes, and the actual or possible results of these operational definitions.

I should like to be permitted one liberty. There is a long and honorable history attached to such words as power, influence, control, and authority. For a great many purposes, it is highly important that a distinction should be made among them; thus to Max Weber, "*Herrschaft ist. . . ein Sonderfall von Macht*," Authority is a special case of the first, and Legitimate Authority a subtype of cardinal significance (11). In this essay I am seeking to explicate the primitive notion that seems to lie behind *all* of these concepts. Some of my readers would doubtless prefer the term "influence," while others may insist that I am talking about control. I should like to be permitted to use these terms interchangeably when it is convenient to do so, without denying or seeming to deny that for many other purposes distinctions are necessary and useful. Unfortunately, in the English language power is an awkward word, for unlike "influence" and "control" it has no convenient verb form, nor can the subject and object of the relation be supplied with noun forms without resort to barbaric neologisms.

POWER AS A RELATION AMONG PEOPLE

What is the intuitive idea we are trying to capture? Suppose I stand on a street corner and say to myself, "I command all automobile drivers on this street to drive on the right side of the road"; suppose further that all the drivers actually do as I "command" them to do; still, most people will regard me as mentally ill if I insist that I have enough power over automobile drivers to compel them to use the right side of the road. On the other hand, suppose a policeman is standing in the middle of an intersection at which most traffic ordinarily moves ahead; he orders all traffic to turn right or left; the traffic moves as he orders it to do. Then it accords with what I conceive to be the bedrock idea of power to say that the policeman acting in this particular role evidently has the power to make automobile drivers turn right or left rather than go ahead. My intuitive idea of power, then, is something like this: *A* has power

subject. The comments of Jacob Marschak on this paper have also been most helpful. There are, of course, approaches radically different from the one employed here and in the works mentioned above. John R. P. French, Jr. (2), has developed a model that assumes "a unidimensional continuum of opinion which can be measured with a ratio scale," and he defines "the power of *A* over *B* (with respect to a given opinion) [to be] equal to the maximum force which *A* can induce on *B* minus the maximum resisting force which *B* can mobilize in the opposite direction." Game theory provides still another approach. Cf. References 4, 5, 9.

over B to the extent that he can get B to do something that B would not otherwise do.

If Hume and his intellectual successors had never existed, the distinction between the two events above might be firmer than it is. But anyone who sees in the two cases the need to distinguish mere "association" from "cause" will realize that the attempt to define power could push us into some messy epistemological problems that do not seem to have any generally accepted solutions at the moment. I shall therefore quite deliberately steer clear of the possible identity of "power" with "cause," and the host of problems this identity might give rise to.

Let us proceed in a different way. First, let us agree that power is a relation, and that it is a relation among people. Although in common speech the term encompasses relations among people and other animate or inanimate objects, we shall have our hands full if we confine the relationship to human beings. All of the social theory I mentioned earlier is interesting only when it deals with this limited kind of relationship. Let us call the objects in the relationship of power, actors. Actors may be individuals, groups, roles, offices, governments, nation-states, or other human aggregates.

To specify the actors in a power relation—A has power over B—is not very interesting, informative, or even accurate. Although the statement that the President has (some) power over Congress is not empty, neither is it very useful. A much more complete statement would include references to (a) the source, domain, or *base* of the President's power over Congress; (b) the *means* or instruments used by the President to exert power over Congress; (c) the *amount* or extent of his power over Congress; and (d) the range or *scope* of his power over Congress. The base of an actor's power consists of all the resources—opportunities, acts, objects, etc.—that he can exploit in order to effect the behavior of another. Much of the best writing on power—Bertrand Russell is a good example—consists of an examination of the possible bases of power. A study of the war potential of nations is also a study of the bases of power. Some of the possible bases of a President's power over a Senator are his patronage, his constitutional veto, the possibility of calling White House conferences, his influence with the national electorate, his charisma, his charm, and the like.

In a sense, the base is inert, passive. It must be exploited in some fashion if the behavior of others is to be altered. The *means* or instruments of such exploitation are numerous; often they involve threats or promises to employ the base in some way and they may involve actual use of the base. In the case of the President, the means would include the *promise* of patronage, the *threat* of veto, the *holding* of a conference, the *threat* of appeal to the electorate, the *exercise* of charm and charisma, etc.

Thus the means is a mediating activity by A between A's base and B's response. The *scope* consists of B's responses. The scope of the President's power might therefore include such Congressional actions as passing or killing a bill, failing to override a veto, holding hearings, etc.

The *amount* of an actor's power can be represented by a probability statement: e.g., "the chances are 9 out of 10 that if the President promises a judgeship to five key Senators, the Senate will not override his veto," etc. Clearly the amount can only be specified in conjunction with the means and scope.

Suppose now we should wish to make a relatively complete and concise statement about the power of individual A over individual a (whom I shall call the respondent) with respect to some given scope of responses. In order to introduce the basic ideas involved, let us restrict ourselves to the 2 by 2 case, where the actor A does or does not perform some act and the respondent a does or does not "respond." Let us employ the following symbols:

(A, w) = A does w. For example, the President makes a nationwide television appeal for tax increases.
$(A, \bar{w})$ = A does not do w.
(a, x) = a, the respondent, does x. For example, the Senate votes to increase taxes.
$(a, \bar{x})$ = a does not do x.

$P\ (u|v)$ = Probability that u happens when v happens.

Then a relatively complete and concise statement would be symbolized:

$$P(a, x|A, w) = p_1$$

$$P(a, x|A, \overline{w}) = p_2$$

Suppose now, that $p_1 = 0.4$ and $p_2 = 0.1$. Then one interpretation might be: "The probability that the Senate will vote to increase taxes if the President makes a nationwide television appeal for a tax increase is 0.4. The probability that the Senate will vote to increase taxes if the President does not make such an appeal is 0.1."

PROPERTIES OF THE POWER RELATION

Now let us specify some properties of the power relation.

1. A necessary condition for the power relation is that there exists a time lag, however small, from the actions of the actor who is said to exert power to the responses of the respondent. This requirement merely accords with one's intuitive belief that A can hardly be said to have power over a unless A's power attempts precede a's responses. The condition, obvious as it is, is critically important in the actual study of power relations. Who runs the XYZ Corporation? Whenever the president announces a new policy, he immediately secures the compliance of the top officials. But upon investigation it turns out that every new policy he announces has first been put to him by the head of the sales department. Or again, suppose we had a full record of the times at which each one of the top Soviet leaders revealed his positions on various issues; we could then deduce a great deal about who is running the show and who is not. A good bit of the mystery surrounding the role of White House figures like Sherman Adams and Harry Hopkins would also be clarified by a record of this kind.

2. A second necessary condition is, like the first, obvious and nonetheless important in research: there is no "action at a distance." Unless there is some "connection" between A and a, then no power relation can be said to exist. I shall leave the concept of "connection" undefined, for I wish only to call attention to the practical significance of this second condition. In looking for a flow of influence, control, or power from A to a, one must always find out whether there is a connection, or an opportunity for a connection, and if there is not, then one need proceed no further. The condition, obvious as it is, thus has considerable practical importance for it enables one to screen out many possible relations quite early in an inquiry.

3. In examining the intuitive view of the power relation, I suggested that it seemed to involve a successful attempt by A to get a to do something he would not otherwise do. This hints at a way of stating a third necessary condition for the power relation. Suppose the chances are about one out of a hundred that one of my students, Jones, will read *The Great Transformation* during the holidays even if I do not mention the book to him. Suppose that if I mention the book to him and ask him to read it, the chances that he will do so are still only one out of a hundred. Then it accords with my intuitive notions of power to say that evidently I have no power over Jones with respect to his reading *The Great Transformation* during the holidays—at least not if I restrict the basis of my action to mentioning the book and asking him (politely) to read it. Guessing this to be the case, I tell Jones that if he does not read the book over the holidays I shall fail him in my course. Suppose now that the chances he will read the book are about 99 out of 100. Assume further that nothing else in Jones's environment has changed, at least nothing relevant to his reading or not reading the book. Then it fully accords with my intuitive notions of power to say that I have some power over Jones's holiday reading habits. The basis of my power is the right to fail him in his course with me, and the means I employ is to invoke this threat.

Let me now set down symbolically what I have just said. Let

(D, w) = my threat to fail Jones if he does not read *The Great Transformation* during the holidays.

$(D, \bar{w})$ = no action on my part.
(J, x) = Jones reads *The Great Transformation* during the holidays.

Further, let

$p_1 = P(J, x|D, w)$ the probability that Jones will read *The Great Transformation* if I threaten to fail him.

$p_2 = P(J, x|D, \bar{w})$ the probability that Jones will read the book if I do not threaten to fail him.

Now let us define the *amount of power*. To avoid the confusion that might arise from the letter p, let us use the symbol M (from *Macht*) to designate the amount of power. Then, in accordance with the ideas set out in the illustration above, we define A's power over a, with respect to the response x, by means of w, as M, or, more fully:

$$M\left(\frac{A}{a}: w, x\right) = P(a, x \mid A, w) - P(a, x \mid A, \bar{w}) = p_1 - p_2$$

Thus in the case of myself and Jones, M, my power over Jones, with respect to reading a book during the holidays, is 0.98.

We can now specify some additional properties of the power relation in terms of M:

a. If $p_1 = p_2$, then $M = 0$ and no power relation exists. The absence of power is thus equivalent to statistical independence.

b. M is at a maximum when $p_1 = 1$ and $p_2 = 0$. This is roughly equivalent to saying that A unfailingly gets B to do something B would never do otherwise.

c. M is at a minimum when $p_1 = 0$ and $p_2 = 1$. If negative values of M are to be included in the power relation at all—and some readers might object to the idea—then we shall have a concept of "negative power." This is not as foolish as it may seem, although one must admit that negative control of this kind is not ordinarily conceived of as power. If, whenever I ask my son to stay home on Saturday morning to mow the lawn, my request has the inevitable effect of inducing him to go swimming, when he would otherwise have stayed home, I do have a curious kind of negative power over him. The Legion of Decency sometimes seems to have this kind of power over moviegoers. Stalin was often said to wield negative power over the actions on appropriations for foreign aid by the American Congress. A study of the Senate that will be discussed later suggested that at least one Senator had this kind of effect on the Senate on some kinds of issues.

Note that the concept of negative power, and M as a measure, are both independent of the *intent* of A. The measure does, to be sure, require one to assign a positive and negative *direction* to the responses of the respondent; what one chooses as a criterion of direction will depend upon his research purposes and doubtless these will often include some idea as to the intent of the actors in a power relation. To take a specific case, p_1 *could* mean "the probability that Congress will defeat a bill if it is contained in the President's legislative program," and p_2 could mean "the probability that Congress will defeat such a bill if it is not contained in the President's legislative program." By assigning direction in this way, positive values of M would be associated with what ordinarily would be interpreted as meaning a "negative" influence of the President over Congress. The point of the example is to show that while the measure does require that direction be specified, the intent of A is not the only criterion for assigning direction.

POWER COMPARABILITY

The main problem, however, is not to determine the existence of power but to make comparisons. Doubtless we are all agreed that Stalin was more powerful than Roosevelt in a great many ways, that McCarthy was less powerful after his censure by the Senate than before, etc. But what, precisely, do we mean? Evidently we need to define the concepts "more power than," "less power than," and "equal power."

Suppose we wish to compare the power of two different individuals. We have at least five factors that might be included in a comparison: (1) differences in the basis of their power, (2) differences in means of employing the basis, (3) differences in the

scope of their power, i.e., in type of response evoked, (4) differences in the number of comparable respondents, and (5) differences in the change in probabilities, or M.

The first two of these may be conveniently thought of as differences in properties of the actors exercising power, and the last three may be thought of as differences in the responses of the respondents. Now it is clear that the pay-off lies in the last three—the responses. When we examine the first two in order to compare the power of individuals, rulers, or states, we do so on the supposition that differences in bases and means of actors are very likely to produce differences in the responses of those they seek to control.

As I have already indicated, much of the most important and useful research and analysis on the subject of power concerns the first two items, the properties of the actors exercising power, and there is good reason to suppose that studies of this kind will be as indispensable in the future as they have been in the past. But since we are concerned at the moment with a formal explication of the concept of power, and not with an investigation of research problems, (some of these will be taken up later on) it is important to make clear that analysis of the first two items does not, strictly speaking, provide us with a comparison of the power of two or more actors, except insofar as it permits us to make inferences about the last three items. If we could make these inferences more directly, we should not be particularly interested in the first two items—at least not for purposes of making comparisons of power. On the other hand, given information about the responses, we may be interested in comparing the efficiency of different bases or means; in this case, evidently, we can make a comparison only by holding one or both of the first two factors constant, so to speak. In general, the properties of the power wielder that we bring into the problem are determined by the goals of one's specific research. For example, one might be interested in the relative power of different state governors to secure favorable legislative action on their proposals by means of patronage; or alternatively, one might be interested in the relative effectiveness of the threat of veto employed by different governors.

In whatever fashion one chooses to define the relevant properties of the actors whose power he wishes to compare, strictly speaking one must compare them with respect to the responses they are capable of evoking. Ideally, it would be desirable to have a single measure combining differences in scope, number of comparable respondents controlled, and change in probabilities. But there seems to exist no intuitively satisfying method for doing so. With an average probability approaching one, I can induce each of 10 students to come to class for an examination on a Friday afternoon when they would otherwise prefer to make off for New York or Northampton. With its existing resources and techniques, the New Haven Police Department can prevent about half the students who park along the streets near my office from staying beyond the legal time limit. Which of us has the more power? The question is, I believe, incapable of being answered unless we are ready to treat my relationships with my students as in some sense comparable with the relations of the Police Department to another group of students. Otherwise any answer would be arbitrary, because there is no valid way of combining the three variables—scope, number of respondents, and change in probabilities—into a single scale.

Let us suppose, for a moment, that with respect to two of the three variables the responses associated with the actions of two (or more) actors we wish to compare are identical. Then it is reasonable to define the power of A as greater than the power of B if, with respect to the remaining variable, the responses associated with A's acts are greater than the responses associated with B's acts. It will be readily seen, however, that we may have jumped from the frying pan into the fire, for the term "greater than" is still to be defined. Let us take up our variables one by one.

To begin with, we may suppose that the probability of evoking the response being the same for two actors and the numbers of comparable persons in whom they can evoke the response also being the same, then if the

scope of responses evoked by A is greater than that evoked by B, A's power is greater than B's. But how can we decide whether one scope is larger than another? Suppose that I could induce my son to bathe every evening and to brush his teeth before going to bed and that my neighbor could induce his son to serve him breakfast in bed every morning. Are the two responses I can control to be counted as greater than the one response my neighbor can control? Evidently what we are willing to regard as a "greater" or "lesser" scope of responses will be dictated by the particular piece of research at hand; it seems fruitless to attempt to devise any single scale. At one extreme we may wish to say that A's scope is greater than B's only if A's scope contains in it every response in B's and at least one more; this would appear to be the narrowest definition. At the other extreme, we may be prepared to treat a broad category of responses as comparable, and A's scope is then said to be greater than B's if the number of comparable responses in his scope is larger than the number in B's. There are other possible definitions. The important point is that the particular definition one chooses will evidently have to merge from considerations of the substance and objectives of a specific piece of research, and not from general theoretical considerations.

Much the same argument applies to the second variable. It is clear, I think, that we cannot compare A's power with respect to the respondents $a_1, a_2 \ldots a_n$ and B's power with respect to the respondents $b_1, b_2 \ldots b_n$ unless we are prepared to regard the two sets of individuals as comparable. This is a disagreeable requirement, but obviously a sensible one. If I can induce 49 undergraduates to support or oppose federal aid to education, you will scarcely regard this as equivalent to the power I would have if I could induce 49 Senators to support or oppose federal aid. Again, whether or not we wish to treat Senators as comparable to students, rich men as comparable to poor men, soldiers as comparable to civilians, enlisted men as comparable to officers, military officers as comparable to civil servants, etc., is a matter that can be determined only in view of the nature and aims of the research at hand.

The third variable is the only one of the three without this inherent limitation. If scope and numbers are identical, then there can be no doubt, I think, that it fully accords with our intuitive and common-sense notions of the meaning of power to say that the actor with the highest probability of securing the response is the more powerful. Take the set of Democratic Senators in the United States Senate. Suppose that the chance that at least two-thirds of them will support the President's proposals on federal aid to education is 0.6. It is fair to say that no matter what I may do in behalf of federal aid to education, if there are no other changes in the situation except those brought about by my efforts the probability that two-thirds of them will support federal aid will remain virtually at 0.6. If, on the other hand, Senator Johnson, as majority leader, lends his full support and all his skill of maneuver to the measure the probability may rise, let us say, to 0.8. We may then conclude (what we already virtually know is the case, of course) that Senator Johnson has more power over Democratic Senators with respect to federal aid to education than I have.

Earlier in defining the amount of power by the measure, M, I had already anticipated this conclusion. What I have just said is precisely equivalent to saying that the power of A with respect to some set of respondents and responses is greater than the power of B with respect to an equivalent set if and only if the measure M associated with A is greater than the measure M associated with B. To recapitulate:

$$M\left(\frac{A}{a} : w, x\right) = p_1 - p_2, \text{ where}$$

$$p_1 = P(a, x|A, w)$$

the probability that a will do x, given action w by A

$$p_2 = P(a, x|A, \bar{w})$$

the probability that a will do x, given no action w by A.

$$M\left(\frac{B}{b}:y, z\right) = p_1^* - p_2^*, \text{ where}$$

$$p_1^* = P(b, z/B, y)$$

$$p_2^* = P(b, z/B, \bar{y}).$$

Now if these two situations are *power comparable* (a notion we shall examine in a moment) then A's power is greater than B's if and only if

$$M\left(\frac{A}{a}:w, x\right) > M\left(\frac{B}{b}:y, z\right).$$

In principle, then, whenever there are two actors, A and B, provided only that they are power comparable, they can be ranked according to the amount of power they possess, or M. But if it is possible to rank A and B, it is possible to rank any number of pairs. And it is obvious from the nature of M that this ranking must be transitive, i.e.,

$$\text{if } M\left(\frac{A}{a}:w, x\right) > M\left(\frac{B}{b}:y, z\right), \text{ and}$$

$$M\left(\frac{B}{b}:y, z\right) > M\left(\frac{C}{c}:u, v\right), \text{ then}$$

$$M\left(\frac{A}{a}:w, x\right) > M\left(\frac{C}{c}:u, v\right).$$

In principle, then, where any number of actors are in some relation to any number of equivalent subjects, and these relations are regarded as power comparable, then all the actors can be unambiguously ranked according to their power with respect to these subjects.

There is, as everyone knows, many a slip 'twixt principle and practice. How can one convert the theoretical measure, M, into a measure usable in practical research? Specifically, suppose one wishes to examine the power relations among some group of people—a city council, legislature, community, faculty, trade union. One wants to rank the individuals in the group according to their power. How can one do so?

The first problem to be faced is whether given the aims, substance, and possible theoretical import of his study, one does in fact have *power comparability*. One of the most important existing studies of the power structure of a community has been criticized because of what appears to have been a failure to observe this requirement. A number of leaders in a large Southern city were asked, "If a project were before the community that required *decision* by a group of leaders—leaders that nearly everyone would accept—which *ten* on the list of forty would you choose?" On the basis of the answers, individuals were ranked in such a way that a "pyramidal" power structure was inferred to exist in the city, i.e., one consisting of a small number of top leaders who made the key decisions, which were then executed by a larger middle-group of subordinate leaders. The significance of this conclusion is considerably weakened, however, if we consider whether the question did in fact discriminate among different kinds of responses. Specifically, suppose the leaders had been asked to distinguish between decisions over local taxes, decisions on schools, and efforts to bring a new industry to the community: would there be significant differences in the rankings according to these three different kinds of issues? Because the study does not provide an answer to this question, we do not know how to interpret the significance of the "pyramidal" power structure that assertedly exists. Are we to conclude that in "Regional City" there is a small determinate group of leaders whose power significantly exceeds that of all other members of the community on all or nearly all key issues that arise? Or are we to conclude, at the other extreme, that some leaders are relatively powerful on some issues and not on others, and that no leaders are relatively powerful on all issues? We have no way of choosing between these two interpretations or indeed among many others that might be formulated.

Let us define A and B as formally power comparable (in the sense that the relative magnitudes of the measure M are held to order the power of A and B correctly) if and only if the actors, the means, the respondents and the responses or scopes are comparable. That is,

the actor	A	is comparable to		the actor	B;
A's respondent,	a,	"	"	B's respondent,	b;
A's means,	w	"	"	B's means,	y; and
a's response,	x	"	"	b's reponse,	z.

But this is not a very helpful definition. For the important question is whether we can specify some properties that will insure comparability among actors, respondents, means, and scopes. The answer, alas, is no. So far as an explication of the term "power" is concerned, power comparability must be taken as an undefined term. That is, power comparability will have to be interpreted in the light of the specific requirements of research and theory, in the same way that the decision as to whether to regard any two objects—animals, plants, atoms, or whatnot—as comparable depends upon general considerations of classification and theoretical import. To this extent, and to this extent only, the decision is "arbitrary"; but it is not more "arbitrary" than other decisions that establish the criteria for a class of objects.

To political scientists it might seem farfetched to compare the power of a British prime minister over tax legislation in the House of Commons with the power of the President of the United States over foreign policy decisions in the Senate. It would seem farfetched because the theoretical advantages of such a comparison are not at all clear. On the other hand, it would not seem quite so farfetched to compare the two institutional positions with respect to the "same" kind of policy—say tax legislation or foreign policy; indeed, political scientists do make comparisons of this kind. Yet the decision to regard tax legislation in the House of Commons as comparable in some sense to tax legislation in the Senate is "arbitrary." Even the decision to treat as comparable two revenue measures passed at different times in the United States Senate is "arbitrary." What saves a comparison from being genuinely arbitrary is, in the end, its scientific utility. Some kinds of comparisons will seem more artificial than others; some will be theoretically more interesting and more productive than others. But these are criteria derived from theoretical and empirical considerations independent of the fundamental meaning of the term power.

On what grounds, then, can one criticize the study mentioned a moment ago? Because the use of undiscriminating questions produced results of very limited theoretical significance. By choosing a relatively weak criterion of power comparability, the author inevitably robbed his inquiry of much of its potential richness. Considerations of comparability are, therefore, critical. But the criteria employed depend upon the problem at hand and the general state of relevant theory. The only way to avoid an arbitrary and useless definition of "power comparability" is to consider carefully the goals and substance of a particular piece of research in view of the theoretical constructs one has in mind. Thus in the case of the Senate, it may be satisfactory for one piece of research to define all Senate roll-call votes on all issues as comparable; for another, only votes on foreign policy issues will be comparable; and for still another, only votes on foreign policy issues involving large appropriations; etc. In a word, the researcher himself must define what he means by comparability and he must do so in view of the purpose of the ranking he is seeking to arrive at, the information available, and the relevant theoretical constructs governing the research.

APPLICATIONS OF THE CONCEPT OF POWER COMPARABILITY

Assuming that one has power comparability, the next problem is to rank every actor whose rank is relevant to the research. Here we run into practical problems of great magnitude.

Suppose we wish to rank a number of Senators with respect to their influence over the Senate on questions of foreign affairs. Specifically, the respondent and response are defined as "all Senate roll-call votes on measures that have been referred to the Foreign Relations Committee." To begin with, let us take two Senators. What we wish to find out is the relative influence on the Senate vote of the activities of the two Senators for or against a measure prior to the roll call. "For" and "against" must be defined by reference to some standard "direction." Passage of the measure is one possible "direction" in the sense that a Senator can be for passing the measure,

against it, or without a position for or against passage. This is not, however, a particularly significant or meaningful direction, and one might wish to determine the direction of a measure by reference to the President's position, or by content, or by some other standard. For this discussion, I shall assume that "for" and "against" are defined by reference to the first standard, i.e., passing the measure.

Let us now assume that a Senator does one of three things prior to a roll-call vote. He works for the measure, he works against it, or he does nothing. (The assumption, although a simplification of reality, is by no means an unreasonable simplification). Let us further assume (what is generally true) that the Senate either passes the measure or defeats it. With respect to a particular Senator, we have the following conditional probabilities:

	The Senator		
	Works For	Works Against	Does Nothing
The Senate Passes	p_1	p_2	p_3
The Senate Defeats	$1 - p_1$	$1 - p_2$	$1 - p_3$

Since the bottom row provides no additional information we shall, in future, ignore it. Following the earlier discussion of the concept M, the measure of power, it is reasonable to define

$$M_1 = p_1 - p_3.$$

$$M_2 = p_3 - p_2.$$

M_1 is a measure of the Senator's power when he works for a measure and M_2 a measure of his power when he works against a measure; in both cases a comparison is made with how the Senate will act if the Senator does nothing. There are various ways in which we might combine M_1 and M_2 into a single measure, but the most useful would appear to be simply the sum of M_1 and M_2. To avoid confusion with the earlier and slightly different measure which we are now approximating, let us call the sum of M_1 and M_2, M^*. Like M, it is at a maximum of 1 when the Senate always passes the bills a given Senator works for and always defeats the bills he works against; it is at a minimum of -1 when the Senate always defeats the bills he works for and always passes the bills he works against; and it is at 0 when there is no change in the outcome, no matter what he does.

In addition, there is one clear advantage to M^*. It is easily shown that it reduces to

$$M^* = p_1 - p_2.$$

In a moment we shall see how advantageous such a simple measure is.

The theoretical problem, then, is clear-cut and a solution seems reasonably well defined. It is at this point, however, that practical research procedures begin to alter the significance of a solution, for the particular operational means selected to breathe life into the relatively simple formal concepts outlined so far can produce rather different and even conflicting results.

Let me illustrate this point by drawing on a paper by Dahl, March, and Nasatir (1) on influence ranking in the United States Senate. The aim of the authors was to rank thirty-four Senators according to their influence on the Senate with respect to two different areas, foreign policy and tax and economic policy. The 34 Senators were all those who had held office continuously from early 1946 through late 1954, a long enough period, it was thought, to insure a reasonably large number of roll-call votes. The classification of measures to the two areas was taken from the *Congressional Quarterly Almanac*, as were the votes themselves. Thus the subject was well defined and the necessary data were available.

No such systematic record is maintained of course, for the positions or activities of Senators prior to a roll-call vote, and what is more it would be exceptionally difficult to reconstruct the historical record even over one session, not to say over an eight-year period. Faced with this apparently insuperable obstacle, it was necessary to adopt a rather drastic alternative, namely to take the recorded roll-call vote of a Senator as an indication of his position and activities *prior to* the roll-call. While this is not unreasonable, it does pose one major difficulty: a vote is necessarily cast either for or against a measure and hence the roll-

call provides no way of determining when a Senator does nothing prior to the roll-call. But the very essence of the formal concept of power outlined earlier hinges on a comparison of the difference between what the Senate will do when a Senator takes a given position and what it does when he takes no position.

It is at this point that the advantages of the measure M^* reveal themselves. For provided only that one is prepared to take the Senator's recorded vote as a fair indication of his prior position and activities, the data permit us to estimate the following probabilities, and hence M^*

		The Senator	
		Works For	Works Against
The Senate	Passes	p_1	p_2

One could, therefore, estimate M^* for each of the 34 Senators and rank all of them.

The validity of this method ranking would appear to be greatest, however, when all Senators are ranked on precisely the same set of bills before the Senate. To the extent that they vote on different (although mostly overlapping) sets of bills, the comparability of M^* from one Senator to another will be reduced, conceivably to the vanishing point.

For a number of reasons, including a slightly different interpretation of the characteristics of an ideal measure, the authors chose a rather different approach. They decided to pair every Senator against every other Senator in the following way. The number in each cell is an estimate of the probability that the Senate will pass a proposal, given the positions of the two Senators as indicated; the number is in fact the proportion of times that the Senate passed a foreign policy (or tax) measure in the period 1946–54, given the recorded votes of the two Senators as indicated.

		S_1	
		Favors the motion	Opposes the motion
S_2	Favors the motion	p_{11}	p_{12}
	Opposes the motion	p_{21}	p_{22}

With 34 Senators, 561 possible pairs of this kind exist; but only 158 pairs were tabulated for foreign policy and 206 for tax and economic policy over the whole period. The measure used to enable comparisons to be made between the two Senators in each pair might be regarded as an alternative to M^*. This measure—let us call it M''—rests upon the same basic assumption, namely that we can measure a Senator's influence by the difference between the probability that the Senate will pass a measure the Senator opposes and the probability that it will pass a measure he supports. However, there are two important differences. First, the authors decided not to distinguish between "negative" and "positive" power; consequently they used absolute values only. Second, in estimating the probability of a measure passing the Senate, the positions of two Senators were simultaneously compared in the manner shown in the table. Thus the influence of S_1 over the Senate was measured as the difference between the probability that a bill will pass the Senate when S_1 favors it and the probability that it will pass when S_1 opposes it. However, this difference in probabilities was measured twice: (1) when S_2 favors the motions before the Senate; and (2) when S_2 opposes the motions. In the same way, S_2's influence was measured twice. Thus:

$$M_1''(S_1) = |p_{11} - p_{12}|,$$

that is, the change in probabilities, given S_2 in favor of the bill.

$$M_2''(S_1) = |p_{21} - p_{22}|,$$

that is, the change in probabilities, given S_2 in opposition to the bill. Likewise,

$$M_1''(S_2) = |p_{11} - p_{21}|$$

$$M_2''(S_2) = |p_{12} - p_{22}|.$$

The influence of S_1 was said to be greater than the influence of S_2 only if $M_1''(S_1) > M_1''(S_2)$ *and* $M_2''(S_1) > M_2''(S_2)$. That is, if

$$|p_{11} - p_{12}| > |p_{11} - p_{21}| \text{ and}$$

$$|p_{21} - p_{22}| > |p_{12} - p_{22}|.$$

Except for the rare case of what would ordinarily be regarded as "negative"

power—which, as I have already said, this particular measure was not intended to distinguish from "positive" power—the absolute values are the same as the algebraic ones. Where the algebraic differences can be taken, and this will normally be the case, both inequalities reduce to

$$p_{21} > p_{12}.$$

In the ordinary case, then, using the measure M'' we can say that the power of Senator George is greater than that of Senator Knowland if the probability that the Senate will pass a measure is greater when Senator George favors a bill and Senator Knowland opposes it than when Senator Knowland favors a bill and Senator George opposes it.

TABLE 1

THIRTY-FOUR U. S. SENATORS RANKED ACCORDING TO "POWER" OVER SENATE DECISIONS ON FOREIGN POLICY, 1946–54

HIGH

Hayden	(tie) Magnuson
	Chavez
	Smith (N. J.)**
	George**
	Maybank
	Green**
	Hill*
Aiken	(tie) Wiley**
	Hoey
	Kilgore
	Ferguson*
	Murray*
	Knowland*
	Morse
Fulbright**	(tie) Saltonstall
	Johnston
	Cordon
	Hickenlooper**
	Ellender
Millikin	(tie) McClellan
	Eastland
	Russell
	Bridges*
	Johnson (Colo.)
	Byrd
	Butler (Nebr.)
	Langer*
	Young
	Capehart*
	McCarran

LOW

** member of Foreign Relations Committee five or more years

* member of Foreign Relatins Committee one to four years

The results, some of which are shown in Tables 1 to 3, are roughly consistent with expectations based on general knowledge.

Note how the formal concept of power has been subtly altered in the process of research; it has been altered, moreover, not arbitrarily or accidentally but because of the limitations of the data available, limitations that appear to be well-nigh inescapable even in the case of the United States Senate, a body whose operations are relatively visible and well recorded over a long period of time.

The most important and at first glance the most innocent change has been to accept the roll-call position of a Senator as an indication of his position prior to the roll-call vote. This change is for most practical purposes unavoidable, and yet it generates a serious consequence which I propose to call the problem of the chameleon. Suppose a Senator takes no prior position on any

TABLE 2

THIRTY-FOUR U. S. SENATORS RANKED ACCORDING TO "POWER" OVER SENATE DECISIONS ON TAX AND ECONOMIC POLICY, 1946–54

HIGH

	George††
	Millikin††
	Ellender
	Byrd††
	Saltonstall†
	Cordon
	McCarran
	Young
	Hoey††
	Maybank
Johnson (Colo.) ††	(tie) McClellan
	Hickenlooper
	Eastland
	Russell
	Smith (N. J.)
	Knowland
	Aiken
	Capehart
	Johnston
	Bridges
	Hayden (tie) Chavez
Butler (Nebr.)††	(tie) Wiley (tie) Ferguson
Langer	(tie) Hill (tie) Murray (tie) Magnuson (tie) Fulbright (tie) Green
	Morse (tie) Kilgore

LOW

†† member of Finance Committee five or more years

† member of Finance Committee one to four years

TABLE 3

THIRTY-FOUR U. S. SENATORS CLASSIFIED ACCORDING TO 'POWER" OVER SENATE DECISIONS ON FOREIGN POLICY AND TAX POLICY, 1946–54

Tax and Economic Policy	Foreign Policy: High influence	Foreign Policy: Medium influence	Foreign Policy: Low influence
High influence	George**†† Hoey†† Maybank	Ellender Saltonstall† Cordon	Millikin†† Byrd†† McCarran Young Johnson (Colo.)†† McClellan
Medium influence	Smith (N. J.)** Aiken* Hayden Chavez	Hickenlooper** Knowland* Johnston	Eastland Russell Capehart* Bridges*
Low influence	Wiley** Hill* Magnuson Green**	Ferguson* Murray* Fulbright** Morse Kilgore	Butler (Nebr.)†† Langer*

** member of Foreign Relations Committee five or more years
* member of Foreign Relations Committee one to four years
†† member of Finance Committee five or more years
† member of Finance Committee one to four years

bill and always decides how to vote by guessing how the Senate majority will vote; then, if he is a perfect guesser, according to the ranking method used he will be placed in the highest rank. Our common sense tells us, however, that in this case it is the Senate that has power over the Senator, whereas the Senator has no influence on the votes of other Senators.

If the reader will tolerate an unnatural compounding of biological and celestial metaphors, a special case of the chameleon might be called the satellite. Although I have no evidence that this was so, let us suppose that Senator Hoey took no prior positions on issues and always followed the lead of Senator George (Table 3). Let us assume that on foreign policy and tax policy, Senator George was the most powerful man in the Senate—as indeed nearly every seasoned observer of the Senate does believe. By following George, Hoey would rank as high as George; yet, according to our hypothetical assumptions, he had no influence at all on George or any other Senator.

The problem of the chameleon (and the satellite) is not simply an artifact created by the method of paired comparisons employed. It is easy to see that ranking according to the measure M^* would be subject to the same difficulties *given the same data*. The formal concept of power, that is to say, presupposes the existence of data that in this case do not seem to be available—certainly not readily available. If one had the kinds of observations that permitted him to identify the behavior of the chameleon or satellite then no serious problem would arise. One could treat chameleon activity as equivalent to "doing nothing" to influence the passage or defeat of a measure. Since, as we have seen, under the measure M^* the column "does nothing" is superfluous, the effect would be to ignore all cases of chameleon or satellite behavior and make estimates only from the instances where a Senator actually works for or works against various bills.

Thus the conceptual problem is easily solved. But the research problem remains. In order to identify chameleon behavior and separate it from actual attempts at influence, one cannot rely on roll-calls. One

needs observations of the behavior of Senators prior to the roll-calls. But if it is true, as I have been arguing, that observations of this kind are available only with great difficulty, rarely for past sessions, and probably never in large numbers, then in fact the data needed are not likely to exist. But if they do not exist for the Senate, for what institutions are they likely to exist?

CONCLUSIONS: A DIALOGUE BETWEEN A "CONCEPTUAL" THEORETICIAN AND AN "OPERATIONALIST"

The conclusions can perhaps best be stated in the form of a dialogue between a "conceptual" theoretician and a strict "operationalist." I shall call them *C* and *O*.

C. The power of an actor, *A*, would seem to be adequately defined by the measure *M* which is the difference in the probability of an event, given certain action by *A*, and the probability of the event given no such action by *A*. Because the power of any actor may be estimated in this way, at least in principle, then different actors can be ranked according to power, provided only that there exists a set of comparable subjects for the actors who are to be ranked.

O. What you say may be true in principle, but that phrase "in principle" covers up a host of practical difficulties. In fact, of course, the necessary data may not exist.

C. That is, of course, quite possible. When I say "in principle" I mean only that no data are demanded by the definition that we cannot imagine securing with combinations of known techniques of observation and measurement. The observations may be exceedingly difficult but they are not inherently impossible: they don't defy the laws of nature as we understand them.

O. True. But the probability that we can actually make these observations on, say, the U. S. Senate is so low as to be negligible, at least if we want relatively large numbers of decisions. It seems to me that from a strict operational point of view, your concept of power is not a single concept, as you have implied; operationally, power would appear to be many different concepts, depending on the kinds of data available. The way in which the researcher must adapt to the almost inevitable limitations of his data means that we shall have to make do with a great many different and not strictly comparable concepts of power.

C. I agree with all you have said. In practice, the concept of power will have to be defined by operational criteria that will undoubtedly modify its pure meaning.

O. In that case, it seems wiser to dispense with the concept entirely. Why pretend that power, in the social sense, is a concept that is conceptually clear-cut and capable of relatively unambiguous operational definitions—like mass, say, in physics? Indeed, why not abandon the concept of power altogether, and admit that all we have or can have is a great variety of operational concepts, no one of which is strictly comparable with another? Perhaps we should label them: Power 1, Power 2, etc.; or better, let's abandon single, simple, misleading words like "power" and "influence", except when these are clearly understood to be a part of a special operational definition explicitly defined in the particular piece of research.

C. I'm afraid that I must disagree with your conclusion. You have not shown that the concept of power as defined by the measure *M* is inherently defective or that it is never capable of being used. It is true, of course, that we cannot always make the observations we need in order to measure power; perhaps we can do so only infrequently. But the concept provides us with a standard against which to compare the operational alternatives we actually employ. In this way it helps us to specify the defects of the operational definitions as measures of power. To be sure, we may have to use defective measures; but at least we shall know that they are defective and in what ways. More than that, to explicate the concept of power and to pin-point the deficiencies of the operational concepts actually employed may often help us to invent alternative concepts and research methods that produce a much closer approximation in practice to the theoretical concept itself.

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Get rid of the old liberals, then; get rid of the soldier in politics; and put the world into the hands of the scientists, the industrial captains and the artists. For the new society was to be organized, not, like Babeuf's, on the principle of equality, but according to a hierarchy of merit. Saint-Simon divided mankind into three classes: the *savants*, the propertied, and the unpropertied. The *savants* were to exercise the "spiritual power" and to supply the personnel of the supreme body, which was to be known as the Council of Newton—since it had been revealed to Saint-Simon in a vision that it was Newton and not the Pope whom God had elected to sit beside Him and to transmit to humanity His purposes. This council, according to one of Saint-Simon's prospectuses, was to be made up of three mathematicians, three physicians, three chemists, three physiologists, three *littérateurs*, three painters and three musicians; and it was to occupy itself with devising new inventions and works of art for the general improvement of humanity, and in especial with discovering a new law of gravitation applicable to the behavior of social bodies which would keep people in equilibrium with one another. (So the eighteenth-century communist philosopher Morellet, in a book called *The Code of Nature*, had asserted that the law of self-love was to play the same role in the moral sphere as the law of gravitation in the physical.) The salaries of the Council of Newton were to be paid by general subscription, because it was obviously to everybody's advantage that human destinies should be controlled by men of genius; the subscription would be international, because it would of course be to the advantage of all peoples to prevent international wars.

—EDMUND WILSON, *To The Finland Station*

STUDIES OF AUTONOMIC RESPONSE PATTERNS: RATIONALE AND METHODS[1]

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This report briefly summarizes the results of eighteen years of research on the measurement of autonomic functions in human subjects during rest, and presents a discussion of hypotheses and methodology for the study of autonomic response patterns.

Most of the report is devoted to a discussion of instrumentation and methods of measurement employed in current investigations. The following variables are measured: electrical resistance of palmar skin; respiration rate; finger, face, and axillary temperatures; heart rate, potentials and rhythms; changes in finger and leg blood volume; systolic and diastolic blood pressures; stroke volume of the heart; pupillary diameter; stomach and lower bowel motility; salivary output; and selected muscle action potentials. Most responses are amplified and recorded continuously and simultaneously by means of chopper type Offner amplifiers. Pupillary diameter and systolic and diastolic blood pressure are time sampled; the former by means of a Bolex motion picture camera, the latter by means of a Gilson automatic blood pressure recorder. Stomach motility is recorded continuously by means of a new method—a magnetometer which remotely detects the movements of a small ingested magnet. Lower bowel motility is recorded by a bipolar electrode.

In 1939 the senior author and his associates initiated work designed to test the following restatement of the Eppinger and Hess hypothesis (3):

> The differential chemical reactivity and the physiological antagonism of the adrenergic and cholinergic branches of the autonomic nervous system permit of a situation in which the action of one branch may predominate over that of the other. This predominance, or autonomic imbalance, may be phasic or chronic, and may obtain for either the adrenergic or the cholinergic system. Autonomic imbalance, when measured in an unselected population, will be distributed continuously about a central tendency which shall be defined as autonomic balance (5).

The work involved the measurement of a number of autonomically innervated functions in a population of children, the intercorrelation and factorial analysis of the data, and the estimation of the obtained autonomic factor for the individual children. Such factor estimates were called "Scores of Autonomic Balance," and their distribution was found to be approximately normal. It was concluded that the original Eppinger and Hess hypothesis was supported, and that it fitted the obtained results better than did the restatement; i.e., the obtained factor could more logically be interpreted as a sympathetic-parasympathetic continuum than as representing relative dominance of adrenergic or cholinergic divisions.

Further studies with children (6, 7, 10)

[1] This investigation was supported (in part) by research grant M-788 from the Institute of Mental Health, National Institutes of Health, Public Health Service.

and with adults (8) led to the following conclusions:

1. Individual differences in autonomic functioning during controlled rest exist in both children and adults.

2. These differences, measured in terms of an autonomic factor, are continuously distributed and are presumed to be normally distributed in an unselected population. Vagotonia and sympathicotonia, as described by Eppinger and Hess, are regarded as representing extremes on such a continuum.

3. The autonomic factor for a given individual tends to remain constant from year to year, but it may be altered phasically or chronically by changes in external or internal stimulation.

4. Autonomic factor scores are related to certain personality patterns and to certain diagnostic categories, such as anxiety psychoneurosis, battle fatigue, and asthma.

5. Autonomic factor scores do not necessarily reflect a consistent pattern of autonomic balance or imbalance. That is, some individuals demonstrate autonomic patterns wherein some functions suggest an apparent dominance of the sympathetic branch and others indicate an apparent dominance of the parasympathetic branch. Individual differences in patterns of action, therefore, are considered to be significant.

6. Autonomic factor scores from resting data are not necessarily related to autonomic reactivity under stimulation.

The last conclusion is based on a study, not yet published, which found that frequencies of symptoms reported under nine autonomomimetic drugs were related to scores of autonomic balance, but that autonomic reactivity under the drugs was related more closely to autonomic reactivity to a placebo. This study and many other observations convinced us that measures of autonomic balance in a resting state must be supplemented by measures of response patterns to stimuli for which the normative response is established, if ANS data are to prove of more than limited usefulness in the prediction of behavior.

Although many investigators have studied reactivity of discrete ANS functions, and a few have used several variables in their studies,[2] the validity of one, or a few, ANS variables as a measure of total ANS response has never been investigated. A comprehensive and systematic research seemed indicated. Accordingly, our present program on "Studies of Autonomic Response Patterns" was undertaken.

This program, which got under way in 1954, has the following specific objectives:

1. To develop practicable techniques of measurement and statistical analysis of autonomic response patterns in man.

2. To investigate resting, primary, recovery, and secondary autonomic response patterns in man to a variety of physical and psychological stimuli in order to determine (*a*) group central tendencies to specific stimuli, (*b*) the nature of individual response pattern differences, and (*c*) the possibility of classifying stimuli according to group response patterns.

3. To investigate the value of autonomic response patterns in standard test situations for the prediction of psychosomatic and other abnormal reactions to subsequent stressors.

4. To investigate the relationship of autonomic response patterns to personality.

5. To test the hypothesis that emotional behavior in man is characterized by at least eight fundamentally different autonomic response patterns.

6. To determine the most significant variables in autonomic response patterns, so that the number of variables in future research may be reduced.

Certain general hypotheses also may be mentioned:

I. Patterns of autonomic response in man demonstrate consistent variation according to the stimuli which elicit them.

II. Differential autonomic response patterns in man provide a basis for an objective classification of stimulus effect.

III. Consistent individual differences in autonomic response patterns to a given stimulus occur in both primary and secondary responses.

IV. Individual differences in autonomic response patterns, supplemented by differences in autonomic balance, furnish a basis

[2] This work is reviewed in connection with other publications from this project.

for the prediction of men who cannot well withstand physical and psychological stress and men who may develop specific psychosomatic and other psychological disorders.

V. Such predictions will afford a background for preventive therapy which will serve to decrease the frequency of psychological disorders.

The long-term objectives of the program are to test the above-stated hypotheses by simultaneous measurement of as many ANS responses as prove practicable, as elicited by a wide range of physical, physiological, and psychological stimuli, with a follow-up study of subjects with respect to life-stress reactions and disorders.

The remainder of this report describes our methods of measurement.

(A) *General Apparatus:* Twelve channels of recording include an 8-channel Offner[3] type D3 Electroencephalograph, three type 133 Offner DC amplifiers, and one special channel. The console accommodates a 24-inch Offner Dynograph (polygraph) which employs 11 standard pen-motors with 6 ohm coils and one special 600 ohm pen-motor. Six paper speeds are available: .125, .25, .625, 1.25, 2.5 and 6.25 cm./sec. Our typical recording speed is .125 cm. per sec., or about 3″ per minute. A special filter switching panel provides for independent variable time constants when recording through the pre-amplifiers of the D3; and the chopper-type amplifiers employed in this apparatus permit drift-free DC recording when the pre-amplifiers are by-passed. Impedance matching in the latter case is accomplished by means of coupling transformers to which a balancing circuit is added to allow manual recentering of the pens. Such couplers are comparable to the Offner Input Coupler, type 9138. The 12th amplifier is specially constructed for the recording of stomach motility. It drives the 600 ohm pen and is described elsewhere (11).

Two other channels of information are used: (*a*) A Gilson[4] Automatic Blood Pressure Recorder (4) provides automatic inflation and deflation of a standard area cuff at any of four time intervals (1″, 5″, 30″, 60″), or by manual control. The pulse sound from the brachial artery is detected by a Brush pressure microphone, amplified, and recorded on chart paper which moves only as the cuff deflates. A second pen records the inflation pressure, which is pre-set, and the curve of deflation. The chart is calibrated in mm. Hg., and the points of *appearance* and *marked muffling* (1st and 4th blood pressure phases) can be read on the deflation curve as systolic and diastolic blood pressure respectively. (*b*) A Bolex, 16 mm. motion picture camera loaded with Eastman cine-kodak high speed infrared film, emulsion # 2183, is used to record changes in the pupils of the eyes and/or facial expression. Two Sylvania infrared self-contained reflector type heat lamps are placed three feet from the subject and directed at his face. The increase in temperature on the face is negligible[5] since the lamps are operated at 55 rather than 110 volts AC. A type 200 C, 5 amp. General Radio Company variac is used to reduce voltage input to the lamps. The camera may be operated manually by remote cable control or by a motor-driven trigger. For most work the latter is employed with one exposure every three seconds. Calibration is accomplished with a black grease pencil mark of known length drawn directly below the subject's left eye.

All of the above apparatus except the camera and its automatic trigger are contained in one room. In an adjoining semi-soundproofed room is a shielded cage (size 9 x 6 x 6 ft.) which houses the cot for the subject and the pick-up units which are described in the following sections. The cot consists of a foam-rubber mattress on a firm wooden base so that no movements of metal structures occur. The head of the cot is adjustable, with hinges at 21 inches from the top. The typical elevation is 6½ inches. (Initial work employed a chaise-lounge with rope support for the pad, which is shown in published pictures of our laboratory [12, p. 268]. It has been supplanted by a structure which is longer, wider, and higher.)

[3] Offner Electronics, Inc., Chicago 25, Illinois.
[4] Gilson Medical Electronics, Madison, Wisconsin.

[5] The temperature at the concentration point of the two lights was found to rise 1° C within ten minutes and to maintain this level for 40 minutes.

A window between the rooms permits observation of the subject, and furnishes reflected light to the subject's room—the only lighting typically employed. An intercommunication system permits constant monitoring of sounds from the subject's room, and communication with the subject when desired. Any or all of the communication may be recorded on a long-playing tape recorder. An air conditioner provides constant room temperature in all seasons.

(B) *Recordable Variables:* Three of the recordable physiologic variables already have been mentioned: Pupillary diameter and systolic and diastolic blood pressure. Others are described in the following paragraphs. All utilize the Offner AC/DC equipment, and are selected according to the experiment.

1. *Electrical Skin Resistance:* For recording skin resistance, a constant current of 40 microamperes is impressed through electrodes made of zinc plates 2 cm. in diameter, encased in a plastic cup. Contact with the skin is made with an agar zinc sulfate electrode paste.[6] A Darrow-type bridge (2) with a range of 500,000 ohms is utilized, and the output is DC amplified. Calibration is determined by bridge regulation.

2. *Respiration:* The pick-up for respiration consists of an aluminum plate .035 inches thick, 5½ inches long and 1½ inches wide. The center is bent into an arch within which a Baldwin SR4 type C-14 strain gauge[7] (nominal resistance 2000 ohms) is cemented. The ends of the aluminum plate are attached to a cloth strip which fastens around *S*'s chest. As respiratory movements bend the plate, the strain gauge is stretched and its electrical resistance changes concomitantly. This variable resistor is one arm of a Wheatstone bridge which is powered by a 22½ volt battery. The output is DC amplified. Amplitude is not calibrated.

3. *Skin Temperature:* Temperatures of the face, finger, and axilla (and room temperature) are recorded through a DC amplifier by means of Western Electric type 14B thermistors. Resistances of the thermistors vary from approximately 2000 ohms at 23° C to approximately 975 ohms at 41° C. Each thermistor functions as one arm of a Wheatstone bridge which is powered by a 1½ volt battery. All temperatures are recorded on one channel by means of a constant speed, motor-driven, selector switch which samples face and axillary temperatures once and finger temperature twice every 10 seconds. Room temperature is selectively recorded by means of a separate, manually operated switch. Calibration is accomplished by substituting known resistances for the thermistors, whose resistances at given temperatures have been predetermined. The sensitivity of each thermistor is controlled by means of a voltage divider from the bridge power supply. Thermistors are attached with adhesive tape which also covers the sensing element, to the right cheek and tip of the second finger of the right or left hand depending upon experimental requirements. A third thermistor, mounted on a sponge rubber pad and placed in the axilla, is held in position by the pressure of the subject's arm. Room temperature is recorded from a free thermistor.

4. *Heart Rate and Variability:* Heart rate appears on several channels. The most dependable measure is the typical EKG obtained from AC recording with standard electrodes. Lead II usually is employed. In some experiments a cardiotachometer[8] is used with DC amplification to measure the period of each heart cycle. The pick-up electrodes are the same as those for the EKG except that lead IV is used to obtain maximum QRS potentials. Condensor discharge time reflects individual heart periods, and determines the deflection of the recording pen. Calibration involves a constant speed motor with a series of contacts designed to establish records equivalent to 30, 90, 120 and 180 heart cycles per minute.

[6] Forty grams of Penick's USP No. 1 powdered agar-agar, 3 grams zinc sulfate ($ZnSO_4$), in 257 ml. of distilled H_2O. The solution, premixed cold in a 600 ml. Pyrex beaker, is placed into a 1" bath of boiling water and cooked with constant stirring for 1 minute and 45 seconds. After cooling it is placed in air-tight jars.

[7] Baldwin-Lima-Hamilton Corp., Philadelphia 42, Pa.; or the entire pick-up unit is available from Electro-Medical Engineering Co., Burbank, Calif.

[8] Constructed by Electro-Medical Engineering Co., Burbank, Calif.

5. *Finger Plethysmograph:* A small plastic cup is placed over the index finger so that 4 cc. of the finger are enclosed. The system is made air-tight with caulking compound. Tygon plastic tubing (size $\frac{3}{8}$" diameter) connects the cup to a P97-0.05D-350 Statham pressure transducer[9] used as a gauge transducer. Blood volume changes in the finger then activate the unit which is powered by a 10 volt battery. Amplification is either DC, as is typical, or AC. The latter has certain advantages and is being investigated (1). A balancing and calibrating circuit described by Statham is utilized. Calibration is accomplished by switching a resistor in parallel with one arm of the transducer bridge. The resulting pen displacement equals that produced by a digital volume change of .01 cc.

6. *Leg Plethysmograph:* A standard blood pressure cuff is placed around the calf of the left leg, and inflated to 20 mm. Hg. Pressure changes from the cuff produced by changes in blood volume of the leg activate a Statham P5-0.2D-1800 differential pressure transducer powered by a 28 volt battery. The output typically receives DC amplification, but AC recording is being investigated.

7. *Stomach Motility:* Only a brief description will be given here since the technique for recording this function is discussed elsewhere (9, 11). The subject swallows a $\frac{3}{16}$" x $\frac{1}{2}$" plastic-coated cast Alnico #5 magnetic rod,[10] the movements of which are remotely detected by a modified Waugh Magnetometer.[11] A magnetic phase detector generates a variable current which is proportional to the lines of magnetic force cut by the magnet. The signal is increased by means of an AC amplifier and then is filtered to give a DC signal. Recording is either by standard Offner DC equipment or with our special amplifier (11).

8. *Muscle Potentials:* Muscle activity of frontalis or of the flexors and extensors of the left forearm is measured in some experiments with silver electrodes, 8 mm. in diameter and 10 cm. apart. Cambridge electrode jelly is used to maximize conductance; and the electrodes are secured to the skin by means of collodion. The Offner D-3 has a built-in calibrator which covers a range of 10 to 1000 microvolts in seven steps. This is utilized to calibrate muscle potentials and other AC signals.

9. *Salivation:* We have made many attempts to record salivation continuously, either by weight or volume displacement. Such measurement requires a wide range of operation with great stability and sensitivity. The most promising technique at present consists of a lever system comprising a steel plate, $8\frac{1}{2}$ inches in length, 3 inches wide, and .08 inches thick, attached solidly to a plastic block on one end and engaging a Statham G1-1-1000 force transducer on the other end. A graduated 100 ml. cylinder is fastened to this plate. One tube from the double-hole stopper closing the cylinder goes to a rubber-tipped saliva ejector which is placed in the subject's mouth, and another goes to a negative pressure pump. As saliva is collected in the cylinder the added weight bends the plate slightly and activates the transducer which is very sensitive, having a maximum range of 2 oz. and a maximum displacement of .003 inches. 20 volts is impressed across the transducer bridge and the output receives DC amplification. A 1000 MFD, 25V Cornell-Dubilier dielectric capacitor is placed across the output to filter out disturbances caused by the negative pressure pump and movements of the subject. The calibrating resistor is chosen to give a pen displacement equivalent to a weight of 2 ml. of H_2O introduced into the collecting bottle. Increases in rate of salivary flow are reflected by a steeper slope of the recorded line.

10. *Ballistic Index of Cardiac Output:* The pick-up for this measure is an Astatic L-12-U crystal phonograph cartridge with a maximal signal output of 4 volts. A 4.3 megohm load resistor is placed across the output terminals. The cartridge is encased in sponge rubber without a phonograph needle and is strapped to the subject's leg six inches above the ankle. The signal receives AC amplification and calibration is in microvolts. The

[9] Statham Laboratories, Inc., Los Angeles 64, Calif.

[10] Terry Sales Corporation, 2910 South La Cienega Blvd., Culver City, Calif.

[11] Now manufactured by E. M. Irwin, 1238 S. Gerhart Avenue, Los Angeles 22, Calif.

record provides a crude measure of cardiac output.

11. *Radial Pulse:* Exploratory work has been done employing a Statham P81-5G-350 transducer placed over the radial artery in an effort to obtain a continuous measure of systolic and diastolic blood pressure. Attachment has been made with rubber sponges and straps, or with a plastic band shaped to curve around the wrist and hold the transducer in place. Initial results with this technique appear promising but a number of improvements are necessary. In particular the system is too sensitive to movements by the subject. A more appropriately shaped transducer and a better method of attachment are indicated.

12. *Motility of the Lower Bowel:* A bipolar electrode of our own design is inserted into the lower bowel through the anus and is held in place by the internal sphincter. Both AC and DC records of changes in potentials have been obtained from a few subjects. If further work indicates that significant data are obtainable by this method the electrode will be described elsewhere.

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The scientist has his guesses as to how the finished picture will work out; he depends largely on these in his search for other pieces to fit; but his guesses are modified from time to time by unexpected developments as the fitting proceeds. These revolutions of thought as to the final picture do not cause the scientist to lose faith in his handiwork, for he is aware that the completed portion is growing steadily. Those who look over his shoulder and use the present partially developed picture for purposes outside science, do so at their own risk.

—A. S. EDDINGTON in *The Nature of the Physical World*

THE COMMUNICATION OF MENTAL HEALTH INFORMATION: A COMPARISON OF THE OPINIONS OF EXPERTS AND THE PUBLIC WITH MASS MEDIA PRESENTATIONS[1]

by Jum Nunnally

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With the advent of television, the impact of information disseminated by such media upon the general public has aroused much interest and speculation. This study presents a comparison of the opinions about mental health problems held by experts (psychologists and psychiatrists) and the general public with the points of view expressed and portrayed in the mass media of communication. A content analysis of the media indicates that it presents ideas about mental health which are further removed from the opinions of experts than are the opinions held by the average man.

THE research reported here is one part of a larger project which concerns the communication of information about mental health to the general public.[2] The project was initiated because there are not now and may not be for a long time, enough professionals in the mental health field—psychologists, psychiatrists, social workers, and others—to perform the services which the public needs. It may be asked, then, whether less direct methods of communication such as that afforded by films, television programs, magazine articles, lectures, and small group discussions, could act as handmaidens to the undermanned professional corps. We were also interested in learning something about the opinions of the general public in order to compare them with the opinions of experts and the media presentations.

One of our initial difficulties was to define what is meant by "mental health information." If one defined mental health as broadly as possible, it would include almost all of human behavior, which would have forced us into studies of recreation, education, marriage adjustment, and many other matters. We, therefore, decided to restrict ourselves to the study of information about the areas of psychotic, neurotic, and mentally deficient behavior. We thought that these would be recognized by everyone as important mental health problems. Any information relating to the causes, symptoms, treatment, prognosis, or social effects of these disorders was considered.

We explored the mental health information that was available in three sources: members of the general public, experts in the mental health field, and presentations of the mass media.

There were several kinds of information that could be studied in our three sources. Chief among these were opinions and attitudes. An example of what we would classify as an opinion would be, "there are more men than women in mental hospitals." This statement could be checked and verified in one way or another. The statement, "I am afraid to be around anyone who has had a 'nervous breakdown,' " would be classified as an attitude. As such, it represents a personal reaction—not a matter for disputa-

[1] The research is being supported by a special grant from the National Institute of Mental Health, U. S. Public Health Service.

[2] Dr. C. E. Osgood is the principal investigator on the project. Members of the project staff in addition to the author are Drs. G. J. Suci, W. L. Taylor, and R. J. Wolff. Our research assistants, Mr. T. R. Husek and Mrs. Carol Tucker, gave very helpful support to the work.

tion. The material reported here will be confined to our studies of opinion.

METHOD

Questionnaire construction. In order to study the correspondence between public information, expert information, and mass media content, the instruments used to sample the information must yield directly comparable data. Our first step was to base the opinion-measuring instrument on a sampling of the content in the three information sources. For the experts, sample opinions were gathered from mental hygiene books, professional publications, and over 200 public information pamphlets.

The samples of public thinking were taken from protocols of 200 detailed personal interviews, eight interviews with mothers of children in psychotherapy, and twelve interviews with the wives of men who were in mental hospitals.[3] Opinions and beliefs expressed in the interviews ranged from a belief that the blood of the insane is blue to a belief that psychiatrists are older than other physicians.

For the media, small samples of the content of newspapers, magazines, and radio and television programs were examined. For example, when the "psychiatrist" in a daytime "soap opera" advised his patient to take a long vacation, this was recorded as an opinion about methods of treatment. In this way we obtained over 3000 statements related to the causes, treatment, symptoms, prognosis, incidence, and social significance of mental health problems.

Because this number of opinions was too large to be dealt with in studies of the public, the sample was reduced to 240 statements removing what appeared to be near duplicates in meaning. These were then cast in questionnaire form, with a seven-point rating scale as shown below:

Most of the insanity cases are found in people over 50 years of age.

Disagree						Agree
1	2	3	4	5	6	7

Mental health is largely a matter of trying hard to control the emotions.

Disagree						Agree
1	2	3	4	5	6	7

The good psychiatrist acts like a father to his patients.

Disagree						Agree
1	2	3	4	5	6	7

The questionnaire was administered to 349 persons in the Champaign-Urbana (Illinois) area chosen to cut broadly across the demographic variables of age, income, sex, education, religion, and marital status. In analyzing the responses it was found that for 60 of the 240 statements almost everyone either agreed strongly or disagreed strongly. Such statements were not included in subsequent analyses.

Factor analysis. In order to distill from the 180 items a smaller number of the major dimensions of mental health opinion, factor analysis was used. The responses to each item were dichotomized, with the cutting point chosen so as to have approximately 50 per cent of the persons below and 50 per cent above that point. Phi coefficients were then computed for the intercorrelations of a 180 by 180 matrix.[4] The coefficients tended to be small with few of them being larger than .40. Owing to the size of the correlation matrix, the square-root method of factoring (sometimes called the diagonal method) was employed (3). Eighteen factors were extracted. Seven of these factors had loadings so small that they were dropped from further consideration. In efforts to interpret the 11 remaining factors, the members of the research team found themselves unable to agree about the interpretations. Moreover, coders attempts to apply the factors to the content of the mass media of communication showed only a low reliability.

Cluster analysis. Rather than undertake the labor of rotating the 11 factors, a cluster-type analysis was performed.[5] The first step was to determine the distances between opinions in the factor space. This was done

[3] For the use of materials employed in the sampling of items, we would like to thank, among others, Drs. Angus Campbell, John Clausen, Elliot Freidson, and Shirley Star.

[4] This work would not have been possible without the help of the Digital Computer Laboratory at the University of Illinois.

[5] Dr. George Suci devised and applied the method of cluster analysis used on our data.

by finding the sum of squared differences in loadings between any two items over the 11 factors. The square root of this quantity is the distance, or *D* measure (1, 2). A cluster was defined as a subset of items for which the within-set distances were consistently smaller than the between-set distances. In other words, a cluster is a set of items located close together in the space defined by the factors and isolated relatively from other items.

Although using the cluster-type analysis afforded a gain in simplicity, it left us with no loadings or other quantitative indices showing the extent to which clusters hang together. Consequently, the clusters should be considered as only roughly indicative of parameters of public opinion. More concrete evidence of the utility of the clusters will be presented later where it will be shown that the clusters correlate interestingly with other variables and thus prove themselves in a direct manner.

Cluster descriptions. This analysis yielded ten clusters, as follows:

I. *Look and act different* (*sub-man*). The mentally ill are beings apart from normal persons, recognizably different in manner and appearance. The mentally ill are thought to have glassy eyes and small brains, to laugh more than "normal" people, and to pay little attention to their personal appearance.

II. *Will power*. Will power is the basis of personal adjustment. Once adjustment is lost, the psychiatrist exercises his own will power to bolster the patient's failing will. Persons who remain mentally ill do not "try" to get better. Most of the people who seek treatment do not need it, and those who do are not very worthwhile persons.

III. *Sex distinction*. Women are more prone to mental disorder than men are. Women worry more than men and more often have "nervous breakdowns."

IV. *Avoidance of morbid thoughts*. Pleasant preoccupation is the basis of mental health. Mental disturbances can be avoided by keeping busy, reading books on "peace of mind," and not discussing troublesome topics. Psychiatrists must have a good sense of humor. The psychiatrist recommends hobbies and other ways for the patient to occupy himself.

V. *Guidance and support*. The maintenance of mental health is through a dependence on strong persons in the environment. The therapist explains to the patient the origin of his troubles and tells the patient where his ideas are incorrect. The mentally ill are persons who lacked affection in childhood.

VI. *Hopelessness*. There is little that can be done to cure a mental disorder. Few of the inmates of mental hospitals return to work in society. Psychiatrists cannot tell whether a condition is curable.

VII. *Immediate external environment vs. personality dynamics*. The individual's state of mental health is dependent on the pressure in the immediate environment. Mental troubles are caused by physical exhaustion, financial and social problems. A cure can be effected by a vacation or change of scenery. The opposite point of view is that the individual's state of well-being is dependent on his history of personal events with special emphasis on the happenings of childhood.

VIII. *Nonseriousness*. Emotional difficulties are thought to be relatively unimportant problems that bring little damage to the individual. Good emotional habits are easy to develop and maintain. This cluster differs from the *hopelessness* cluster in that the *hopelessness* cluster concerns itself with the likelihood of recovery and the value of treatment regardless of the extent to which the problems are damaging. In illustration, an individual may have a disorder, such as an allergy, which would be hopeless in the sense of being incurable, but not serious in the sense of being debilitating.

IX. *Age function*. This is the belief that persons become more susceptible to emotional disorders as they grow older—an apparent analogy with the increasing susceptibility to some of the "physical" disorders. Children are less affected by frightening experiences. Older persons are more prone to insanity and recover more slowly from "nervous breakdowns."

X. *Organic causes*. Mental disorder is brought on by organic factors like poor diet and diseases of the nervous system. It is

associated with physical symptoms like brain lesions and is to be cured by "physical" means.

As the clusters are described they sound like popular misconceptions. As in all factor analysis problems, there was a choice as to which pole of each cluster would be used in the interpretation. This wording was chosen merely to simplify later analyses. It would have been as easy to reverse most of the interpretations, e.g., *nonseriousness* to *seriousness*. Then the clusters would tend to sound more like "correct" beliefs and opinions.

APPLICATION OF THE OPINION CLUSTERS TO THE THREE INFORMATION SOURCES

Public opinion. The sample of responses obtained from the Champaign-Urbana area was re-scored in terms of the clusters. This procedure consisted of finding the mean score for each person on the several items which best represented each item cluster. In addition, a revised form of the opinion questionnaire was constructed containing only the items which characterized the clusters. The new form, containing 50 items, was administered to a sample of 200 respondents in Knoxville, Tennessee. An area sampling of that city was used to obtain an approximately representative cross-section of the population.[6] A third sample of 151 subjects was obtained in Eugene, Oregon.[7] These subjects were restricted in age range, consisting mostly of couples with young children. Otherwise they were spread broadly across the demographic parameters. Some population characteristics of the three samples combined are listed in Table 1. The intercorrelations of four of the demographic variables are shown in Table 2.

TABLE 1
DEMOGRAPHIC CHARACTERISTICS OF THE COMBINED SAMPLES OF PUBLIC
(N = 700)

Characteristic	Percentage of Sample
Age	
10–19	7.3
20–29	20.1
30–39	30.9
40–49	20.1
50–59	10.7
60–69	6.9
70–79, 80–89, 90–99	4.0
Years of education	
2–3	.3
4–5	1.6
6–7	2.0
8–9	10.4
10–11	11.9
12–13	31.1
14–15	10.6
16–17	22.7
18–19	9.4
Annual income (in 100's of $)	
0–12	7.1
12–36	18.5
36–60	30.2
60–84	17.0
84–108	7.8
108–up	11.4
Marital status	
Married	79.0
Single	14.0
Widowed/Divorced	7.0
Religious preference	
Protestant	79.9
Catholic	7.3
Jewish	2.7
None of these	10.0
Sex	
Male	47.8
Female	52.2

Experts. A revised form of the 50-item questionnaire was used to study the opinions of mental health experts. It contained the same items as the questionnaire used for the public but instead of asking the expert what he personally believed, he was asked to indicate what he thought the public *should* be told. Consequently the rating scale for each item was as follows:

The good psychiatrist acts like a father to his patients

Repudiate					Support	
1	2	3	4	5	6	7

The experts were told that the center of the continuum, the 4 point, is used to indicate

[6] We would like to thank Dr. William Coleman, University of Tennessee, for directing the sampling of subjects in Knoxville.

[7] Dr. Richard Littman, University of Oregon, was kind enough to administer our questionnaire while pursuing his own studies of parental attitudes and opinions.

TABLE 2
INTERCORRELATIONS OF FOUR DEMOGRAPHIC VARIABLES

	Education	Age	Sex	Income
Education	—			
Age	.06	—		
Sex	−.16	.02	—	
Income	.29	.04	−.09	—

that a particular notion should be neither supported nor repudiated but ignored in public information efforts. However, a subsequent small study indicated that the experts responded mainly in terms of what they personally believed to be true and false. For this study a group of 40 persons in psychology, psychiatry, and related fields were given the "agree-disagree" form of the questionnaire which had been used with the public. They were asked to judge what the average person in their profession believed about each of the items. These ratings proved to be very much the same as the "repudiate-support" ratings obtained in the major expert sample. The same conclusions would have been reached if either of these sets of ratings had been used in subsequent comparisons.

Our expert sample was drawn from psychologists and psychiatrists in the United States. One hundred and fifty psychologists, who are members of the American Psychological Association and who have been awarded diplomas by the American Board of Examiners in Professional Psychology, were sent questionnaires by mail. Half were sampled from the diplomates in clinical psychology and the remainder from diplomates in counseling and guidance. One hundred and fifty psychiatrists, drawn from the Group for the Advancement of Psychiatry, were mailed questionnaires. Thus the expert sample was chosen in such a way as to obtain what most mental health professionals would consider "qualified" persons.

Cooperation of the experts was good, with 201 of them eventually replying. The first 176 replies were used in the analysis. This included the responses of 90 psychiatrists and 86 psychologists.

Mass media. The questionnaire study used with the general population and the experts could not be used for the media. Rather, the item clusters were used as coding categories in a content analysis of the media. For example, if an individual's psychotic behavior is explained in a comic strip as having resulted from a "bump on the head" this would be coded as an instance of the *organic causes* cluster. Three coders were used to judge each item, with an item unit being defined as an article in a newspaper and magazine and as a separate program on radio and television. Only codings on which two of the three coders agreed was included in the results.[8] This empirical derivation of a set of coding categories is a departure from traditional approaches to content analyses, where categories are usually established on an a priori basis.

The sample of newspapers consisted of 323 issues, divided among 50 newspapers of three circulation sizes, published during the month of October, 1954, and giving a broad coverage across the United States. The magazine sample consisted of all the issues on the racks of several large newsstands in the Champaign-Urbana area. In all, one issue of each of 91 magazines was studied. The television sample, consisting of all programs on one station over the period of a week, was the most laborious and expensive to study, and consequently the sample of content was rather limited. The sample of radio content was small but reasonably representative, consisting of all the offerings for one week of four stations in widely scattered geographic regions and affiliated with four different networks.

RESULTS

One of our prime considerations was to determine whether or not the individual's opinions about mental health problems are related to his age, income, education,

[8] Dr. Wilson Taylor had chief responsibility for carrying out the media analysis. He is presently preparing a full account of that work for publication.

TABLE 3
CORRELATION OF OPINION CLUSTERS WITH FOUR DEMOGRAPH VARIABLES*
(N = 700)

Cluster	Education	Age	Sex†	Income
I. Look and Act Different	−.40	.16	.08	−.21
II. Will power	−.35	.18	−.03	−.28
III. Sex distinction	−.05	.06	.06	.01
IV. Avoidance of Morbid Thoughts	−.24	.38	.10	−.17
V. Guidance and Support	−.15	.09	.18	−.09
VI. Hopelessness	−.27	.22	−.03	−.20
VII. Immediate external environment vs. Personality Dynamics	.02	.16	.10	−.01
VIII. Non-Seriousness	−.30	.30	.08	−.14
IX. Age Function	−.31	.18	−.05	−.22
X. Organic Causes	−.21	−.02	−.11	−.16

* The standard error of the correlation coefficient with degrees of freedom of 697 is .038. Correlations as large as .10 are beyond the .01 level of significance.

† Point-biserial correlations were computed between sex and opinion cluster. A positive correlation means that women are more prone to believe the particular cluster.

and other demographic characteristics. Each person was given a score on each opinion cluster by averaging his scores on the items belonging to the cluster. Cluster scores were then correlated with demographic data. Separate correlations were determined for the Champaign-Urbana, (Illinois), Knoxville, (Tennessee), and Eugene, (Oregon) samples. The three sets of correlations were then averaged. The average correlations are presented in Table 3.

As the clusters are oriented, they tend to correlate negatively with years of education. In particular, people who have more years of education reject the *look and act different* cluster, the belief that *will power* is the mainspring of mental illness, and the belief that older persons are more susceptible to emotional disorders. Only clusters III and VII correlate insignificantly with education.

The clusters tend to correlate positively with the age variable. When the correlation between age and education is partialed from the correlations of age with the opinion clusters, the correlations change by less than one point. This indicates that younger persons and older persons with the same number of years of education have different opinions about mental health problems.

The correlations with sex are too large to be easily ascribed to chance and too small to be of much explanatory importance. There is a slight tendency for women as a group to reject *organic causes* in favor of explanations in terms of the *immediate external environment*, and vice versa for men as a group. The *avoidance of morbid thoughts* and the *guidance and support* of others are accepted by women more than by men.

When the contingency between income and education is partialed, the remaining correlations between income and the opinion clusters are too small to be of practical significance but statistically significant in several instances. The people of higher income tend to reject the *will power* cluster and the *age function* cluster. They are also more prone to regard the outlook for the mentally ill as hopeful.

Experts. One often hears it said—and by professionals in this area—that the so-called experts on mental health and illness cannot agree among themselves. This may be so for the finer points of theory and techniques, but it is not so for the kinds of information that the public at large deals with, at least as indicated by our data. The item variances for the experts were in all cases lower than those for the general public and were less than one-half as large on the average. Another way of showing the extent of agreement among experts is in relation to the percentage of persons who disagree with the majority decision on each item. If more than 50 per cent of the subjects mark that an item should be advocated to the public, then the percentage of persons who mark one of the "repudiate" categories, positions 1, 2, and 3 on the scale, indicates the percentage of disagreement with the majority decision. The experts show less than 10 per cent disagreement on half of the 50 items. The general public shows at least this amount of disagreement on all of the items. The average responses by psychiatrists and by the two groups of psychologists were much alike. In no case was the difference between the means of any two groups on any of the clusters greater than one-third scale unit.

Mass media. Most of the material in the content analysis of the mass media occurred incidentally in the presentations rather than

as designated mental health information. That is, it is more likely to see material relevant to psychotherapy in one of the evening drama programs on television, than in a documentary on the treatment of psychological disorders.

The amount of information in the media about mental health problems is apparently not large. The amount estimated would, of course, depend on the kinds of problems studied; we have restricted ourselves to only the major problem categories. Programs relative to mental health problems appeared about 3.1 per cent of the time. Although we have no strictly comparable data, rough checks indicate that materials bearing on "physical disorders"—heart trouble, cancer, poliomyelitis—would occur from four to ten times as often.

Comparison of experts, public, mass media

The experts and the public could be compared directly on the opinion clusters, but the data obtained from the mass media content analysis had to be converted to the seven-point scale form. As the data came from the analysis they consisted of ratings by coders of the number of times that one pole of a cluster was portrayed as opposed to the frequency for the other pole. For example, 80 instances were found in which the *immediate external environment* was portrayed as being at the root of particular disorders. The opposite pole of the factor, *personality dynamics*, was portrayed only 29 times. This gave a 74 per cent occurrence for cluster VI. Percentages of this kind then were converted to a seven-point scale. This allows a direct comparison of the results from the mass media with the opinions given by the experts and the public. It was necessary for us to assume that the relative frequency with which a cluster is mentioned in the media is equivalent to the intensity with which the cluster is portrayed. This might not be strictly so in some scaling problems. The first step in converting the media data to the seven-point scale was to subtract the percentage of times that a cluster appeared from 50. This number was then multiplied by six and the resulting figure was added to the number four. The rationale for this procedure is the assumption that the divergence of a frequency from 50 per cent demonstrates the extent to which the frequency should be scaled away from the neutral (4 position) toward either the 1 or the 7 position.

The scaling procedure was tried on the questionnaire responses of the public, where we had both frequencies and mean responses. The scaling method duplicated the mean responses very well, being different in no case by more than one-third scale unit and having an absolute deviation on the average of only one-tenth scale unit. This gave us some confidence in applying the scaling method to the frequencies obtained from the media analysis. However, such fine points of scaling are not required in examining the research results. The divergence of the media from both experts and the public is so large as to be apparent without a careful statistical scrutiny.

Figure 1 shows the mean scores for the two groups on each of the clusters. The scores for the mass media are shown by heavy dots. Three clusters (will power, sex distinction, and age function) occurred an insufficient number of times in the media—less than ten occurrences—to permit a comparison with the opinions of experts and the public. Comparing the profile of opinions for the experts and the public, it is apparent that the average layman is not far different from the average expert. The profile for the public is generally to the right of that for experts, toward the "incorrect" side of each cluster as we had arranged them; but the differences between the two profiles are not large.

Experts and laymen (the arithmetic means of both samples), *agree* with the following opinions:

1. Early training will *not* make the child's brain grow faster.
2. Most of the people in mental hospitals speak in words that can be understood.
3. Children usually do *not* forget about frightening experiences in a short time.
4. X rays of the head will *not* tell whether a person is likely to develop insanity.

The laymen, like the experts, preponderantly disagree with statements like the following:

1. There is not much that can be done for a person who develops a mental disorder.

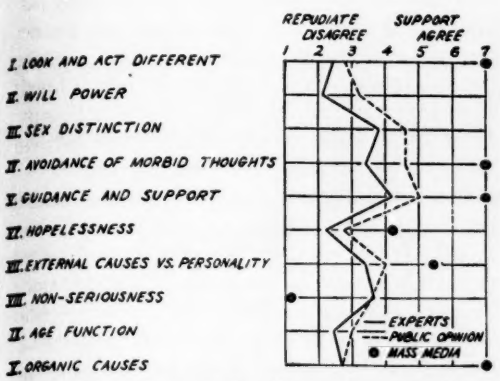

FIG. 1. Comparison of the Mean Cluster Ratings Obtained from Experts, General Public, and the Mass Media.

2. Most people who "go crazy" try to kill themselves.

3. If a child is jealous of a younger brother it is best not to let him show it in any way.

4. Almost any disease that attacks the nervous system is likely to bring on insanity.

The average man and the average expert disagree most on the techniques required to maintain personal adjustment and to restore personal adjustment once it is lost. In terms of particular items, the most pronounced differences are as follows:

1. Books on "peace of mind" prevent many person from developing nervous breakdowns. (experts repudiate, public agrees)

2. If a person concentrates on happy memories, he will not be bothered by unpleasant things in the present. (experts repudiate, public agrees)

3. The main job of the psychiatrist is to explain to the patient the origin of his troubles. (experts repudiate, public agrees)

4. Good emotional habits *cannot* be taught to children in school as easily as spelling can. (experts support, public disagrees)

5. When a person is recovering from a mental illness, it is best not to discuss the treatment he has had. (experts repudiate, public agrees)

6. A person *cannot* rid himself of unpleasant memories by trying hard to forget them. (experts support, public disagrees)

A look at Figure 1 shows that the mass *media* are not *mediating* between experts and the public. In general, the causes, symptoms, methods of treatment, prognosis, and social effects portrayed by the media are far removed from what the experts advocate. In particular the media in their over-all presentations emphasize the bizarre symptoms of the mentally ill. In the television drama program, for example, the afflicted person often enters the scene staring glassy-eyed, with mouth widely agape, mumbling incoherent phrases, or laughing uncontrollably. Even in what would be considered the milder disorders, neurotic phobias and obsessions, the afflicted person is presented as having bizarre expressions and actions.

The occurrence of mental disorder is explained in the media most often by pressures in the immediate external environment. The "soap opera" heroine develops a neurosis because her husband dies in a plane crash, her little daughter is afflicted with an incurable disease, and all the family savings are lost in a fire. The "neurosis" goes away with a brighter turn of events. If the pressures of the immediate external environment are not brought in as causal explanation, *organic* factors are cited. A magazine fiction story might explain neurotic or psychotic behavior in terms of an old battlefield injury, a head wound in childhood, or physical privation such as thirst or hunger.

In the media, the person with a mental disorder most often receives help from some strong person in the environment who lends *guidance and support*. The "strong" individual may be a person who is professionally trained—a psychiatrist, priest, or nurse; equally often the guiding hand is that of a homespun philosopher who manages to say the right thing at the right time. Such cogencies as, "The world is what you make of it," "The past cannot hurt you," are portrayed as profoundly influencing the course of the mental illness.

DISCUSSION

The data presented here concern public opinions (as defined earlier) not the more affective or attitudinal type of response. Moreover, the kinds of opinions were relatively simple and truncated. We could only work with statements in a language that the

public understands. The domain of opinions which we have studied appears to be relatively unstructured. Correlations were low between the questionnaire items, and the factors were weakly defined. Coupled with the results from the media, which show a relatively low proportion of mental health content, this finding suggests that mental health is not discussed enough for people to form stable and organized opinions.

The correlation of the opinion clusters with amount of education suggests that more education will bring more movement in a desirable direction, and perhaps that relatively simple programs of communication can serve to increase the public information.

There are two ways in which we can explain the tendency for older persons to hold viewpoints that are judged by experts to be more unacceptable. One explanation is that the aging process, and the expected changes in personality, affect the opinions which are held. Another explanation, one to which we are more inclined, is that older persons and younger persons of the same years of schooling received different kinds of education. The social studies are given more emphasis now in schools, and it is also possible that mental health topics are more often discussed in younger age groups.

Outside of the older age group and the low education group (more than 50 years of age and less than a high school education), the population responses were not markedly divergent from what the experts advocate. There are substantial areas of disagreement, but at least the public rejects the superstititions and more flagrant misconceptions. The public disagreed most with the experts on the same issues in which the experts disagreed most among themselves, regarding techniques for maintaining and restoring personal adjustments. There are, of course, many conflicting points of view among psychiatrists and psychologists about theories of personality and of psychotherapy. It is to be expected then that the layman will also be confused on these issues.

One crucial question is why mental health problems are treated as they are in the media. The media are, of course, commercial ventures whose policies and presentations are determined in part by their internal needs. Perhaps it is necessary to emphasize the bizarre symptoms in order to make the presentations more exciting. Some of the simpler explanations given for the genesis and treatment of mental disorders may be due to the need to get the programs going and over with in a relatively narrow time span or printed space. If the media took time to illustrate the complexities of the learning process that experts deem to be the important components in personality disorders, they might have some very dull programs. In any event, our results indicate that the presentations of mental health problems by the mass media do not agree with the opinions of experts nor even with those of the public.

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(Manuscript received January 4, 1957.)

COMMENT

An Exchange of Views on Psychic Energy and Psychoanalysis

March 23, 1956

Dr. K. M. Colby,
2107 Van Ness,
San Francisco, Calif.

Dear Dr. Colby:

Thanks for the book on Energy and Structure. It is the most careful and insightful analysis of the basic assumptions of psychoanalysis that I have seen. It also reveals most clearly what have seemed to me to be the weaknesses and fallacies of the system; which have made me regard Freud's formulations as scientific nonsense. Chief among these are:

1. The classification of instincts or drives in terms of end results. This has always been characteristic of the literature on instinct. It would do no harm, if recognized as nothing but taxonomy, but it has always led, especially in Freud, to a tacit assumption that the activities included in a given category have similar mechanisms. The dynamic theory has grown out of such assumptions; the energies within a category are free to shift from one activity to another. Mastication and conversion of sugars are nutritive functions; therefore both are due to the pressure of anabolic energy. Much of the logic of psychoanalysis seems to me to be of like character. I discussed this in 1924 (*Psychol. Rev.*, p 172).

I have always been sceptical of theories of mental or nervous energy. McDougall's drainage theory, Spearman's theory of *G*, Köhler's differences of cortical potential, Halstead's interpretation of frontal lobe defects—ENERGY is postulated whenever a quantitative variable is found. Most of the phenomena ascribed to it can be accounted for in terms of variations in excitability or threshold. This is certainly true of hormone action. Male and female behavior in mating and reactions to young can be independently induced by different hormones and the drive is not interchangeable among these activities. The work of Stone, Beach and others of my students indicates that what is affected by the hormones is the excitability by specific stimuli.

As you say, the psychoanalytic system is based on energy concepts and I do not believe that the data justify them. There is no known source of energy of such character in the nervous system. I discount the recent claims for the "centrencephalic" system. Such facilitative functions as have been shown for it are too diffuse to provide reinforcement of specific acts.

2. Preoccupation with problems of neurosis caused Freud to overlook the fact that the mechanisms which he postulated to account for them are universal in behavior.

a. No psychological or neural process of organization is conscious. I tried to illustrate this in the Hixon symposium.

b. Repression occurs in every act of attention, in the maintenance of set or of a connected discourse.

Restriction of these concepts to psychopathology and the injection of moral considerations led to the notion of limited sources of energy and the personification of those sources. It is more probable that any dominant habit system can play the part of "censor"; contaminations of which Freud made so much, occur at all levels of organization, even in conflict of reflexes in the spinal animal. The elaborate schemata of the *Traumbedeutung* and *Psychopathologie des Alltagslebens* are quite unnecessary.

3. The problem of validation of evidence. I hoped to find a discussion of this in your first chapter but it seems never to be raised by analysts. About 1928 I asked Franz Alexander to discuss the question in my seminar in Chicago. Amusingly, he introduced his talk by stating that psychoanalysis uses exactly the same methods of validating data as do other sciences; then devoted the rest of the hour to discussing my motives in asking him to talk on that subject. Ten years later I arranged a seminar

with Sachs at Harvard on the same topic. Sachs turned out to be a lovable, *gemütlich* Austrian with literary and artistic tastes but not the faintest notion of what constitutes scientific proof. From the literature and my analytic acquantances I have been forced to conclude that the observations are so contaminated by theory that it is impossible to discover the actual facts with which analysis deals.

I am afraid that your book has only confirmed my belief that psychoanalysis is the best therapy for the psychiatrist that he can use on his patients. But thanks, again.

Sincerely yours,
K. S. LASHLEY

Yerkes Laboratories of Primate Biology,
Orange Park, Florida

20 April 56

Dear Dr. Lashley:

It was very kind of you to devote so much time and thought to *Energy and Structure.* Rarely is an author paid the compliment of such detailed comment on his work.

Many of your points are well taken while others ("Freud's formulations are scientific nonsense") seem quaint. It is true that psychoanalysis, as an undeveloped science, suffers from various childhood maladies such as wild speculation. I'm enclosing a reprint of a review in which I too am critical of these trends. But let me take up your comments as you numbered them.

(1) To classify drives according to end results or aims is of course quite unsatisfactory. They should be classified according to origin, i.e., according to those physiological processes which stimulate the CNS to function in a certain way in regulating the behavior of the organism. This is a problem only physiologists are equipped to solve, not psychoanalysts. Psychoanalysts observe total behavior and make guesses as to what drive requirements underlie it at any given moment. These guesses are crude and probably wrong. But until physiologists help us out with a more accurate classification, we have to struggle along with empirically derived notions. For the analyst is a professional, i.e., a practical man who would rather have some working hypothesis than none at all. And there is always the chance that the old scientific saw, "wrong hypotheses, rightly worked, etc." may have something to it.

Psychic or cathexis energy is our rough way of talking about certain electro-chemical activities of the CNS. It is an abstraction on a level of integration higher than cells. We have the right to use such a construct just as the neurophysiologist has the right to think of cellular processes in the bloodbrain barrier while at a lower level of integration molecular deformations take place. "Variations in excitability" would mean that at one level of stimulation energy is not released but when a threshold is exceeded energy is released. If someone doesn't like the words energy or excitation he will invent some other term. But I'll bet you stick with "energy," excitation, and Co. to connote states of activity versus states of rest.

(2) That the same mechanisms operate in health as well as in disease is no mystery to all pathologists, both physio- and psycho-. Everyone carries pathogenic organisms in his throat but not everyone develops, say, streptococcal pneumonia. This does not negate the role of strep in the production of disease. *Certain* strep under *certain* conditions in *certain* people produce pneumonia. Likewise *certain* repressions under *certain* conditions in *certain* people lead to psychopathological symptom formations.

You are quite accurate in stating that the function of repression in itself is not a psychopathological phenomenon. It was discovered by psychopathologists but we now know it to be a part of everyday psychic life.

(3) Scientific method has roughly three phases: (1) Observation of data (2) Construction of hypotheses (3) Validation of hypotheses. Believing in the division of labor, I limited my book to phase (2).

I'm sure you are not serious in saying that analysts are "never" concerned with validation. There is a long literature on the subject and I will be glad to send you 20 or 30 of the better references if you want them. Alexander was happily correct in stating that we

use the same methods of validation as do other sciences. We *attempt* the same methods but would not be so brash as to believe that we are as successful at it as the physical sciences. Perhaps, out of desperation, we will come across some new methods of validation. There is no single way of validating an entire body of theory. We must use various methods depending on the nature of the hypothesis or proposition we are testing. Let me mention two groups of validating methods.

(a) Empirical observation In a single instance we observe recurring regularities (as in the communications of a patient), formulate a hypothesis, make a prediction and then check the prediction by a future observation. For example, I observe a man in the interview situation, lying on a couch free-associating. During the first two weeks, each time he begins to talk about his childhood he falls asleep. He does this regularly, without fail, four times. I formulate a generalization, "If in the analytic situation this man approaches the subject of his childhood, then he will fall asleep." I wait for a further opportunity to validate this proposition which is a prediction in the sense that it tells me what to expect. The fifth time he comes to talk of his childhood he falls asleep and he does so regularly for 26 times. This is a very simple descriptive hypothesis in which the roles of observation, construction and validation are clear.

In the case of multi-instance validation we utilize statistical observations. Here we observe recurring regularities among many individuals. For example, we collect dreams from several hundred men and women, and count the incidence of certain elements to see if a hypothesis regarding sex differences can be confirmed. The greatest difficulty in these statistical studies lies in establishing definable psychic entities. A science doesn't get very far unless it can standardize its definitions sufficiently that specific observations can be identified and specific determinate procedures can be stated. It is no secret that psychoanalytic theory is riddled with unchecked speculations (I have contributed some myself). Consensus definitions of standard units or invariants will help us proceed further with statistical methods.

Besides single– and multi–instance observation we now are aided by cross cultural studies in confirmations of hypotheses. The literature along these lines is already robust.

(b) Experiment. The beauty of experiment lies in the manipulation of variables. Repeatability, predictability, and manipulation are optimally combined in experiment. In psychoanalysis the experiment is sometimes carried out by fate, for instance when children lose their mothers in the first year of life and must live in a hospital. With the elimination of such an essential variable, a standard pathological effect (depression) develops from this experiment of nature.

The crucial experiment of raising children without human contact has yet to be carried out. Then all environmental variables could be controlled and specific variables could be introduced at specific times. But at present the mothers of America would demand the head of such an experimenter and he would suffer for his "inhuman" conduct. But someone with courage will yet make the attempt.

Laboratory experiments with animals have provided some validations and some refutations of psychoanalytic hypotheses. See Hilgard, E. R., *Psychoanalysis As Science.*

Clinical single-instance experiments are conducted daily by the therapist in the form of interpretations in the interview situation. However the multiple variables involved and the lack of controls make the validation of interpretations a special problem.

You mentioned that you can't separate observation from theory in psychoanalysis. This is hard in many sciences at certain times but if you had an opportunity to observe within the analytic situation I think the distinction might become clearer. Or nowadays one has the opportunity to listen to tape recordings of interviews which make clear what the patient says (observations) and how the analyst interprets it (theory). After hearing many tape recordings one becomes convinced that the conditions of the analytic situation are explicit and reproducible.

Now I do hope that I have not convinced you that there is a grain of something scientifically valuable in psychoanalytic efforts. Because psychoanalysis is becoming far too respectable and accepted for my comfort. We need severe and resourceful critics who know what they are talking about, particularly along the proliferating edge of research. On one side of the integrative scale we are related to neurophysiologists and on the other side to anthropologists. All three are converging in their interests, language, and concepts. Since our exchange of letters might be of interest to those involved in this trend, would you have any objection to publishing them? I know of a journal which might have room for them.

Thank you again for your forthright views. They helped me to think.

Regards,
K. M. COLBY

San Francisco, California

Jan. 28, 1957

Dear Dr. Colby:

It is clear, as you have recognized, that psychoanalysis as a theory of mental organization stands or falls with its assumptions concerning the nature of psychic energy or CE. I have been interested for a good many years in questions of nervous and mental energy, as formulated in theories of brain action, motivation, drives and learning and I am convinced that such concepts cannot be justified on any grounds. Major objections to the constructs are the following:

1. The concept of energy, as used in the physical sciences, is defined in terms of mass and acceleration, independently defined. The various forms of energy (heat, electricity, sound, gravitation, etc.) are mutually translatable and are described in terms of structure or organization of more elementary mass movements (oscillation of electrons, e.g.). The characteristics or laws of each form of energy are rigidly determined by the structure, just as the mode of energy transformation of a gas engine is determined by the arrangement of pistons and crankshaft.

2. The nervous system is such a limiting structure. Neural activity has been sufficiently well explored to rule out such broad assumptions as of the energy of the libido or of the id. Summation, potentiation, irradiation, and inhibition are fairly well, though not completely, understood. The energy of the nervous system is that of transmitted excitations, with its implied limitations and specificities. Energy dissociated from this, as postulated in field theories, is ruled out by definite experimental evidence. In particular, the derivation of psychic energy from one or a few "instincts" finds no support in the nature of neural activity. I think you have realized this, on other grounds, as implied in your ascription of cathexis energy to the integrative systems themselves. I think you have thereby eviscerated Freud, if you have not yet buried him.

3. Where instinctive activities have been analyzed experimentally, as in our studies of hunger, mating and maternal behavior, there is nothing that suggests free or transferable energy. There are variations in thresholds of activity with consequent differences in the arousing stimuli, dominance of a reaction system with suppression of other activities, and changes in the effective stimulus and the pattern of response due to observable formation of associations. The behavior can be explained without assumption of any energy other than the interaction of specific neural elements.

There has never been a serious attempt to evaluate the basic postulates and many *ad hoc* assumptions of psychoanalysis in relation to neural mechanisms. From that point of view many of the problems of personality structure and of neuroses assume a different aspect. In general, the effect is to emphasize the need to study each individual as a distinct problem in terms of his own history, in contrast to the analytic procedure of assigning him to one of a few stereotypes set up to correspond to the limited number of postulated sources of psychic energy. (I am aware that, just as the original formal interpretation of dreams of the *Traumbedeutung* has been abandoned, so many of the other formalizations of the early analysts are being dropped. The result may well be

that while Freud still remains the grandfather of psychoanalysis, none of his genes will have survived.)

4. There is little evidence that the energy theories have had any predictive or heuristic values. The rare predictions of analysis are based on simple inference from past parallels, not on deductions from theory. You cite a case in point; the patient who went to sleep when he talked of his childhood; a valid inference but certainly not predictable from energy theory. Of course discoveries arising directly from theory are rare in science, so rare that the prediction of Pluto and of the neutrino stand out almost as unique examples. The chief function of scientific theories is to determine the direction of interest and research and to give coherence to discoveries once made.

If the postulates of a theoretical system are correct, it is a valuable research tool but when they are wrong, it may seriously retard the development of science. The Ptolemaic system and phlogiston theory are outstanding examples of the latter. In the early days of science the spinning of elaborate theoretical systems without testing basic assumptions was excusable but today, with scientific methods well thought out, there is no justification for it. The great danger of such theoretical constructs is that they acquire authority by virtue of their complexity and many subordinate assumptions. Their advocates are likely to mistake support of some minor assumption as proof of the whole system. Many of the arguments for the validity of the psychoanalytic system remind me of a conversation that I recently overheard; recent archeological discoveries confirm historical references in the old testament, therefore the biblical account of creation is true. (I am now living in the bible belt but I have lived in the Boston Psychoanalytic belt.)

This brings up again the question of the validation of psychoanalytic theory. You list the most important procedures but I can find little evidence that they have been applied to the basic problems. (Your example of children reared without their mothers, aside from the fact that institutional rearing introduces many other, perhaps more important variables, is irrelevant to analytic theory.)

The points in the theory which must be validated are: 1, the dynamic or energy assumptions; 2, the assumptions concerning the unitary structure and function of the unconscious, ego, id, etc.; 3, the reliability of the analytic method as a means of research. As I have read the analytic literature, its "scientific" methods seem to me to have been predominantly the following:

1. The method of striking illustrations. The coincidence of two impressive events is observed and a causal relation inferred between them. This is the method of pseudoscience, of phrenology, physiognomy. By it Freud derived the primary data from which he constructed his theories and he never advanced beyond the method of illustration.

2. The method of selected coincidence. Once a theory concerning the cause of certain symptoms was formulated, the analytic procedure consisted of following free associations, dreams, etc. until finally some memory or pseudomemory was elicited which conformed to expectation of the theory. Apparent confirmation by independent observers follows this pattern. Clark and I detected a similar procedure in the mapping of cerebral architectonic areas by followers of Brodman. I devised a simple test of the reliability of observation, the use of which by anatomists now seems to have reduced the number of recognized areas by about 95 per cent. After Jones published his paper on the determination of spontaneous recall of numbers I tried the experiment of taking numbers from Barlow's tables, giving them to subjects and then following up free associations. They frequently led to emotionally toned memories which Jones' logic showed were responsible for the "recall."

Your suggestion that listening to analyses will convince one of their validity leaves me unmoved. I have heard the same suggestion concerning spiritualistic séances. I have waded through a few verbatim reports of analysis which did not increase my respect for them. A similar argument is that one cannot appreciate analytic procedures

until he has been analyzed. But analysis is not complete until one has suffered conversion, after which objective judgment becomes difficult.

3. The principle of liberality (opposite of parsimony). Never consider a simple explanation in preference to a more fanciful one. Much material was available on the confusion of concurrent impulses in action, both in normal and brain injured patients. (Such as the doubling of the wrong letter in typing, Spoonerisms, aphasic confusions). It provided a basis for a simple physiological theory of contamination, adequate for the phenomena in dreams and in the psychopathology of every day life. But Freud had to preserve the ancient struggle of good and evil genii.

4. The Janus theory. Facts seeming to contradict a theory are met, not by questioning the theory, but by claiming that the same cause can produce opposite effects. The doctrine of overcompensation illustrates this. It is not restricted to psychoanalysis. Pavlov published a lovely paper on the inhibition of inhibition (*Schweizer Archiv.*) which is never cited by his followers.

5. Validation by therapeutic success. This is of course quite irrelevant. In preparation for a lecture on the history of psychotherapy I once listed the methods for which there is some evidence of success, from Jesus to Coué, snake oil to Lourdes, Greatrakes to Perkins' tractors, animal magnetism to psychoanalysis, and sought the common elements making for success. The conclusions were amusing, but that is another story.

I might add to this list but it is sufficiently representative of the *typical* procedures of analytic verification. I would appreciate some of the "better" references to which you refer, if they are relevant to the points which I listed above. They may make me modify my "never" but the ratio of one per thousand still approaches zero.

General statements concerning psychoanalysis are likely to lead to misunderstanding, for the system has come to embrace almost the whole of psychiatry, including much that is unrelated to the original construct. Analysts certainly deal with many important facts but these are handled in such a way that it is difficult to cull them from the mass of interpretation. I confess that I have paid little attention to the later developments of the system. I was impressed by it at first (1912–20) and followed the literature rather closely until 1930. Eventually I decided that other forms of science fiction are more rewarding and in the past 25 years have only occasionally sampled the analytic literature, without finding any significant improvement in method or content, however. I have tried here to consider what seems to me the essence of the system; the speculations concerning energy, the reification of instinctive and social processes, and the use of a method in which the personal equation of the investigator plays a large but undetermined role. These seem to be the common elements in the many and varied formulations which lay claim to the title. And I don't like them because they provide facile "explanations" for problems which still demand intensive study.

I value your book because it does deal clearly with the problem of the origin and classification of cathexis energy and seeks a more rational derivation for it. All power to you.

Sincere regards,
K. S. LASHLEY

March 4, 1957

Dear Dr. Lashley:

Your absorbing letter was certainly worth waiting for since it unwaveringly comes to grips with the crucial problems psychoanalysis must meet in attempting to develop as one of the natural sciences. Many of your penetrating comments I must entirely agree with, while in the case of others, I would like to offer some perhaps feeble but nonetheless sincere objections. I will number my paragraphs to correspond with the numbers of your pages and text.[1]

Pg. 1, 1. [234] The physical sciences are not my specialty, but aren't you confusing the concept of energy with the concept of force? In classical mechanical physics force is defined in terms of mass and accelera-

[1] Ed. note—So that the reader may more readily follow the text, bracketed references are given to identify pages in this issue where these points are mentioned.

tion and $\frac{1}{2}mv^2$ is proportional to only the kinetic energy. In modern physics the rest energy can be defined in terms of mass times a constant ($E = mc^2$) or, as in quantum theory, as the frequency of emission of quanta times Planck's constant ($E = h\nu$). When dealing with radiation energy the momentum of a particle associated with a wave (say a photon) is expresed as $p = \frac{h}{\lambda}$ while its energy $= h\nu$. The momentum of a photon can also be stated as $\frac{h\nu}{c}$. Force is proportional to change in momentum and can be equated with kinetic (not total) energy. Such marvelous equivalences should make both neurophysiologist and psychoanalyst blush at the primitiveness of their formulations.

What does all this have to do with concepts of the electro-chemical energy of the brain or the cathexis energy of the central psychic apparatus? One consequence is that we can conceive of our energy distributions and exchange in terms of frequencies, resonance and wave lengths rather than in terms of force, velocity and mass of classical physics.

Pg. 1, 2. [234] We both agree that cathexis energy cannot be limited to an "id" or "instincts." Whether or not this eviscerates Freud, I'm sure he couldn't care less at the moment.

Perhaps I can make clearer how the term "cathexis energy" is used in psychoanalytic theory. We are not assuming some new type of energy independent of the energies known to other sciences. Cathexis energy is purely a *metapsychological construct* and not an observable. It is a way of co-ordinating and talking about certain observations regarding human thought and behavior. We cannot divorce cathexis energy from central structure since structure determines the way in which energy operates.

If we range the sciences along an integrative scale from least complex to most complex organization, the biological level is just beneath the psychological level. In speaking of cathexis energy we are "really" talking about the energy of neurones. And when the neurophysiologist speaks of a nerve impulse, he is "really" talking about the chemistry of a nerve fiber and the fluid around it. And when a biochemist speaks of the chemical energy of living cells, he is "really" talking about electron valences. At the next lowest level, when he talks about electrons as particles with mass and charge, the physicist is "really" talking about a wave phenomenon with no rest mass at all. So in one sense we are all talking about the same ontological "thing" at different conceptual and methodological levels.

The psychoanalyst has every right to formulate a concept of energy in building hypotheses even though he cannot quantify it. We can speak of cathexons as quanta of energy and assume variations of frequency, synchrony and dysynchrony. Instead of calling it simply energy, we give it a special name, not only to convey the idea that it is a construct, but also to suggest that special forms of energy relationships may be involved. An electron in a cell may behave differently from an electron in an iron bar, and an electron in a neuron may act different from an electron in another cell and an electron in a neuron which stores experience may be still different. As we ascend the stairs of doubt from electron to cathexon, we need special terms to indicate that special properties are involved at each level of organization.

With a construct of cathexis energy psychoanalysts can conceptualize change, motion, and activity within the central apparatus. It is intriguing to me to follow the speculations of quantum physicists regarding activity of h in $h = \frac{E}{\nu}$. h is not only a number but some definite abstract mathematical entity of action. Thus there may be atoms of action in nature as well as atoms of matter. At the psychoanalytic level, where we do not use concepts of mass, matter or inertia but of cathexis energy and central structure, we can make good use of some such notion as "atoms of action."

Pg. 1, 3. [234] In the central apparatus there can be no "free" energy in the sense of not being associated with some structural unit. If a structural unit such as a message is in motion relative to macro-systems,

moving from one system to another, then its cathexis is said to be in the "free" state. If a structural unit such as a schema is at rest relative to macro-systems, then its cathexis is said to be "bound."

One recent attempt to relate some psychoanalytic hypotheses to neural mechanisms can be found in the papers of Ostow, *Psychoanalytic Quarterly*. 1954 (p 317–338) and 1955 (p 383–423).

Pg. 2, 4. [235] It's correct that our theory of cathexis energy has no predictive value. Predictions are based on our knowledge of structural content, the registrations of the past experiences of the individual. But all hypotheses in science need not have predictive value. Cathexis energy belongs to that group of notions which have some explanatory value. And what is an explanation for one man is no explanation for another.

"Who is to explain the explanation?" In science it seems we "explain" by expressing the mysteries of *A* in terms of the mysteries of *B* and so on. But good explanations thus have creative and heuristic value since they suggest something beyond themselves.

I would not scold wrong postulates for anything since in themselves they are innocent. It is the people who cling to them, refuse to alter them and make a creed of them who must be blamed. (You have my sympathy for having lived in both Belts). The Ptolemaic and phlogiston theories, while wrong, served a purpose for their time and stimulated at least some people to seek refutations of them.

Each branch of science advances historically in a different way and at a different rate. Physics is now in a highly advanced state (relative to other sciences) since it studied simple problems and had the benefit of thousands of workers in the field for hundreds of years. It took 40 generations of alchemists until a science of chemistry emerged in the 18th century. Even then chemistry was 100 years behind physics. If one science becomes advanced it doesn't mean that all others around it will become advanced by using its methods. Compare physics and physiology as they stand today. The biological sciences are dealing with much more difficult problems than physics faced and the psychological sciences with even more complex phenomena. We shouldn't expect them to equal the precision and effectiveness of physics for a long time.

The early historical stages of a science are characterized by crude observation, arbitrary experiment, and preliminary speculation. The early concepts in physics were clouded with metaphysical and theological issues just as modern cosmology and psychoanalysis are today. It takes time to clarify what are the significant variables and to design suitable experiments in which these variables are studied while others can be held constant or rendered incidental. Psychoanalysis is only 60 years old and just beginning to emerge from the naturalistic stage of grouping observations and speculations. As yet we have no carefully controlled experimental methods and no mathematical language in which we can rigorously state our theories. As yet there is no division into experimenters, theoreticians and clinicians. There is not a single full-time research psychoanalyst in this country! I don't think we are going to get very far until we have dozens and hundreds of researchers as in the other sciences. One can't be a clinician all day and have enough energy (cathexis and otherwise) left over at night to research.

It is easy to compare psychoanalysis with other wrong bodies of theory in the past. One can do it with any current theory in any science. Posterity is sure to consider our gropings in nuclear physics as alchemistic as we view the work of Paracelsus. Our only answer to this and to the sad research state of psychoanalysis is to keep working away at trying to improve our scientific efforts and taking the scoffing pokes as something that goes with this particular territory. I don't feel too badly anymore when I read the names they call cosmologists and the antimatterites.

Pg. 3, 1. [235] Besides being a method of phrenology, it is a method of science. If regularities can be found in the relations between two events which can be repeatedly observed, then a general underlying principle can be attempted, often, but not necessarily, in causal terms. The point is, the two events have to occur more than once

and the evidence has to justify a causal inference. (Please don't ask me what causality is because I would try to answer). Sometimes psychoanalysis follows this procedure, but at other times its hypotheses are of the type "day causes night because night regularly follows day." I wouldn't want to have to defend the worst in my field.

Pg. 3, 2. [235] I didn't want to suggest that listening to analyses convinces one of the "validity" of certain hypotheses. In listening, one (a) observes what hundreds of others have reported as observing, thus meeting the criterion of repeatability of observation, and (b) decides whether or not a given hypothesis is credible or useful in understanding these observations. Many people simply deny the observations made in analysis. To them I would say, "if you don't believe people think and talk and act in such and such a way, then listen to these tape recordings." Once they have listened, I would ask them, "How would you coordinate these observations, with what sort of hypotheses or principles could you make them intelligible?" After they have tried, I would tell them the hypotheses that have been proposed and let them decide on the relative probability or credibility of these hypotheses. There is really nothing arcane about it at all.

There is no need to be analyzed to understand general psychoanalytic theory. We should keep clear that there are three branches of psychoanalytic theory—(1) a theory of general human behavior, (2) a theory of psychopathology, (3) a theory of technical measures designed to modify psychopathology. To grasp (1) requires no experience other than reading. To understand (2) and (3) and their clinical applications, however, one needs experience in observing pathological formations and in trying to modify them.

Pg. 4, 3. [236] Simplicity is a matter of taste. The heavy-supper theory of dreams is just too simple for me. I have yet to hear of a physiological theory of dreams which can explain the content of visual images. To say that neurones aren't working right or that there is a spill-over from one circuit to another is too global, non-specific and unheuristic for me. To explain the precise content of a dream, I would prefer to have a knowledge of structural content at a psychological level, i.e., past experiences of the individual registered as sets of meanings and operating now to influence present thought and behavior.

There is a simple theory that the universe is composed of four elements—fire, air, earth and water. I prefer the more fanciful theory of elements as stated in the periodic table. On the other hand, I prefer a simple theory of determinism to the fanciful (to me) views on indeterminism of quantum physics. I'm sure neither of us would want to reject a theory *only* because it does not meet with "common sense" expectations. All new theories seem bizarre at first.

Pg. 4, 4. [236] Too true to be good. Again, let's not blame the theories for what people do with them. We have to calibrate for human needs in science as well as anywhere else.

Pg. 4, 5. [236] Also true. (There is a growing danger of our agreeing on things here.) Therapeutic effectiveness does not confirm hypotheses. But it stimulates us to seek what it is that brought about this effect. If people are cured by Lourdes or quacks, this fact should not be dismissed but studied for this is a tremendous achievement.

There is no shortage of oafs in any field and every idiom has its idiot. For every third-rate piece of work, I could quote you in psychoanalysis, you could quote me one in neurophysiology. But it does no good to be critical of mediocre minds.

The *ratio* of 1 per thousand approaches zero but 1 *in* 1000 is not something less than 1, but 1. If there is one correct solution found in 1000 tries, then there is one and not a fraction of one. One correct solution is worth thousands of efforts. Think of the thousands of false starts, wrong answers and faulty hypotheses proposed before a Newton, a Mendel or a Darwin came along. Hundreds of great mathematicians before Gauss failed to solve the problem of the parallel postulate. The work of any one of these men represents one in tens of thousands yet we are thankful for that one.

Although we are called soothsayers and

necromancers as the early chemists were called, we aspire gropingly to scientize psychoanalysis. We extend generalizations until they fail and then try new ones. We patiently collect data, speculate at night and correct in the daytime in preparation for a Newton. Besides the scorn outside our field to stimulate us, we have the literati, merchants, doctrinarians and school-marms within who resent such efforts. But they are not serious enemies of promise. We need more workers, time, and allies. I look to the neuropsychologists and neurophysiologists as allies since we are so closely related on the integrative scale of organization, both dealing with neuronal activity at different methodological levels. Although you have not followed psychoanalytic efforts for many years, the fact that you take this trouble to tell me what you think I find most encouraging. That you have not closed your mind, that you are willing to listen and debate—what greater appreciation of effort can one ask?

Dr. J. Miller of *Behavioral Science* has asked me to send him your permission to publish our letters. Either way, yes or no, is all right with me. I have enjoyed the exchange of ideas tremendously and hope one day we may meet to continue it in person.

Regards,
K. M. COLBY

March 9, 1957

Dear Dr. Colby:

I have no objection to publication of our correspondence. If we had had that in view we would probably have tried to be more systematic and less informal, and have produced something much worse. I have violent opinions concerning your interpretation of E and h in modern physical equations but they are aside the main issues. Since I started the discussion, you ought to have the last word. I am content to rest with your last letter.

Sincerely yours,
K. S. LASHLEY

What Is Behavioral Science*

The expression *behavioral science* has come into use in recent years. This designation appears to be an outgrowth of the interest of the Ford Foundation in Program V, "scientific activities designed to increase knowledge of factors which influence or determine human conduct, and to extend such knowledge for the maximum benefit of individuals and of society." This area has been repeatedly referred to as "behavioral science" in more recent writings, and recently a journal with this name was founded.

Behavioral science would be equated by some to psychology. Besides limiting behavioral science unduly, this would not suit some psychologists, inasmuch as it would appear to overemphasize the behavioristic approach. The recently instituted journal is an interdisciplinary effort, for the editorial staff includes not only psychologists and psychiatrists but also a political scientist, a neurophysiologist, an economist, a mathematician, and an educationist. The interdisciplinary aspect of this venture seems clearly desirable.

The question that I wish to raise before the term *behavioral science* takes on an unfortunate restricted meaning is that of the status of genetics, biochemistry, and biophysics. My concern is not prompted by a desire to emphasize a mechanistic approach to the subject, but I have real misgivings about attempting to build a superstructure without any concern for the foundation.

It is no secret that the trend in the social sciences is environmentalistic. When it comes to an interdisciplinary study as broad as behavioral science, however, geneticists need to be called into action because no one can question but that there is an interplay between genetic and environmental influences, and that in order to understand either, one must understand both.

Behavioral science certainly has its roots in biology, and the foremost frontiers of biology lie in biochemistry and biophysics. To leave biochemistry and biophysics out of behavioral science is to be superficial and

* Reprinted from *Science*, volume 124, August 10, 1956.

hedge it about on the basis of a priori assumptions which are quite unwarranted. It is preposterous in view of all we know to exclude nutrition and endocrine balances from the "factors which influence or determine human conduct." It would seem very unfortunate just when genetics is beginning to throw light on these subjects to invite it to stay out.

One of my own interests in this field is related not to the uniformity of human behavior but to the nonuniformities. It is interesting to know as much as we can about why people act alike, but it is also worth while (and crucial in my opinion) to know why people *do not behave alike*. Biochemistry has much to offer in the way of insight into this problem, as is brought out in a forthcoming book on *Biochemical Individuality*.

If biochemistry and genetics were minor disciplines and could contribute only in a trifling way to behavioral science, their exclusion would not be so serious. Very recently, in a principal address at the Chicago meeting of the American Psychiatric Association, Percival Bailey, a neurosurgeon, neurologist, and psychiatrist, indicated that future progress in dealing with mental disease is largely in the hands of biochemists. This bears out the crucial need for interdisciplinary study of behavioral science in which biochemistry is an important part. In line with this need, we have recently instituted, with the support of the Welch Foundation of Houston, a cooperative study at the University of Texas and the Austin State Hospital (for mentally ill) in an attempt to discover the biochemical roots of mental disease.

ROGER J. WILLIAMS

Department of Chemistry and Biochemical Institute, University of Texas, Austin

I agree with the point made by Roger J. Williams. The term as it applies to the Center for Advanced Study in the Behavioral Sciences covers all scientific efforts directed toward an understanding of human behavior. Last year, the fellows of the center included three biologists. Among the current seminars is one on the biological bases of human behavior. One of the biologists who will be a fellow next year is a geneticist. Many scientists studying human behavior recognize the interdependence of biochemical, biophysical, and social factors. More studies in these areas are needed and should be encouraged.

Among the difficulties of unraveling these important interrelated factors are those of developing productive interdisciplinary efforts. Biochemistry and biophysics have developed after many years of unrelated work done by biologists, chemists, and physicists. Psychologists, anthropologists, and sociologists have increasing intellectual contact among themselves but carry on little intensive work with biochemists and biophysicists. I hope that the kind of cooperative study mentioned by Williams will be one of many which will pave the way for fruitful interdisciplinary work involving scientists from both biological and social fields.

RALPH W. TYLER

Center for Advanced Study in the Behavioral Sciences, Stanford, California

Where there is much desire to learn, there of necessity will be much arguing, much writing, many opinions; for opinion in good men is but knowledge in the making.

—John Milton

ABSTRACTS OF CURRENT LITERATURE WITH INTERDISCIPLINARY IMPLICATIONS

Prepared by Members of the Staff of the Mental Health Research Institute

Cartwright, Dorwin, and Harary, Frank. **Structural balance: a generalization of Heider's theory.** *Psychol. Rev.*, 1956, 63, 277–293.

Heider has analyzed certain aspects of cognitive fields which contain persons and impersonal objects or events into *P-O-X* units consisting of *P* (one person), *O* (another person), and *X* (an impersonal entity). Each relation among parts of such units is conceived as interdependent with each other part, and if *X* is evaluated as bad, a state of imbalance occurs and it is hypothesized that pressures arise to change it to a state of balance by altering any of the relationships involved. Two sorts of relations are distinguished, one of attitude such as liking (which is written as *L* for its presence and $\sim L$ for its absence) and the other of cognitive unit formation such as causality, belonging, or similarity (written as *U* to indicate presence and $\sim U$ to indicate absence). A balanced state is defined in terms of combinations of these relations.

This concept of balance contains ambiguities and applies to a limited range of situations.

The mathematical theory of linear graphs may be applied to the problem of defining "balance" in such a way as to "(*a*) encompass unsymmetric relations, (*b*) hold for units consisting of any finite number of entities, (*c*) preserve the distinction between the *complement* and the *opposite* of a relation, (*d*) apply to relations of different types, and (*e*) serve to characterize cognitive units, social systems, or any configurations where both a relation and its opposite must be specified."

Heider's terms *entity*, *relation*, and *sign of a relation* may be coordinated to the graphic terms *point*, *directed line*, and *sign of a directed line*. The concept "cycle" and "semi-cycle" are also used, and the concept "degree of balance" is introduced. Theorems are derived which may be applied to the potential influence structure of a group whose liking relationships are known, to communication networks, power systems, and structural aspects of other empirical configurations. Consequently this constitutes a theoretical approach to quantitative group dynamics. (*JM*)

Dobzhansky, Theodosius. **What is an adaptive trait?** *Amer. Naturalist*, 1956, 90, 337–347.

The failure to see the adaptive significance of traits in which species of animals and plants differ could be interpreted as a measure of our ignorance. This argument is supported by the fact that traits at one time believed to be neutral were later discovered to have an adaptive value. However, it seems very unlikely that all or most of the traits which vary between and within species are directly useful to their possessors. The problem, the author points out, lies in the fact that too often one forgets that the isolation of "traits" is only a semantic device useful in recording discrete observations in the continuum of individual and phylogenetic development. As a result, "traits" are misleadingly thought of as having independent existence, as the stones of a mosaic or the pieces of a jigsaw puzzle. Natural selection, the author stresses, does not operate with separate traits. The success, in the struggle for life, of a particular genotype is determined by the totality of the traits it produces in a given environment. Traits need not by themselves be of any particular importance to the organism, but the developmental processes, of which such trivial traits are the concomitants, may be quite important. A trait, then, which per se does not have any adaptive significance, may be established or eliminated by natural

selection, because it is part of a genetic "syndrome," which contains also selectively effective traits. Often, the more striking characteristics we happen to observe in living beings are less important for survival than the less obvious genetic and developmental correlates of these characteristics.

Examples from genetics are given to support these points, or rather to give impetus to the theoretical discussion: e. g., the presence of one proclinate (bent forward) and two reclinate (bent backward) orbital bristles in more than 600 known species of the genus *Drosophila* (a character hardly important enough to justify its being retained so tenaciously), or the recent findings that the white-eyed mutant of *Drosophila melanogaster* is at a disadvantage in its struggle for survival not because the lack of eye pigment makes it blind, but chiefly because the females, both red-eyed and white-eyed, accept the advances of normal red-eyed males rather than of the white-eyed males. (*RT*)

George, F. H. **Logical networks and behavior.** *Bull. Math. Biophysics*, 1956, 18, 337–48.

In this paper the author studies a class of logical networks (called *B*-nets), and interprets their behavior in terms of his general psychological theory, the main postulates of which have been discussed in a preceding publication but are briefly restated for the reader's convenience.

The behavior of each net can be represented by a series of equations of the McCulloch-Pitts type, each equation indicating under what conditions an element of the network, or "neuron," will or will not fire at a given instant of (quantized) time. The theory can also be formulated using matrix algebra.

Generally speaking, the problem treated is of the input-output type; the author investigates what pattern of stimulation (including "motivation") is necessary and sufficient to elicit a range of "beliefs," and thus a range of "expectancies" in the organism or the corresponding network. Possible extensions of this theory are suggested. (*ET*)

Grinker, R., et al. **A theoretical and experimental approach to problems of anxiety.** *A. M. A. Arch. Neurol. Psychiat.*, 1956, 76, 420–431.

The several authors, collaborators on problems of psychosomatic research at the Institute for Psychosomatic and Psychiatric Research in Chicago outline an experimental design aimed at studying the variations and interrelations of certain selected classes of variables presumed to play important roles in so-called "free anxiety," presumably identifiable as an affect of interpersonal behavior. This affect is taken to be experienced as an "inexplicable foreboding of danger or disintegration."

The following variables have been assumed by the authors to be important correlates of free anxiety.

1. *Affective variables* (anxiety, anger, depression), rated by trained personnel in specially devised evaluative interviews and through one-way screen observations.

2. *Perception and decision*, involving time and accuracy of judgment in simple visual tasks.

3. *Physiological measures*, heart, respiratory rates, etc.

4. *Hormonal measures*, e.g., plasma hydrocortisone and serum protein-bound iodine as measures of adrenocortical and thyroid activity.

It is contemplated to put the subjects through four continuous days of testing involving experimental intervention (i.e., stress stimuli) in three of the four classes of variables, namely in the affective variables through a stress interview, in perception and decision by impugning the accuracy of the subjects' perception, and in the hormonal measures through intravenous injections of corticotropin. The empirical data will consist of more or less continuous measurements on the variables accessible to such measurement before and after the intervention of a particular kind.

The underlying theoretical orientation discussed in the first part of the paper views anxiety as effecting a sudden change of parameters which determine the organized functioning of the organism. It is further recognized that an anxiety producing event

may have repercussions spread over considerable time stretches and that anxiety viewed as a process may consist of intricately interrelated "subsystems." The authors hope that their experimental design will help identify these subsystems, the functional and time-sequential relations among them, and will thus reveal anxiety as an "organizing principle" of behavior under certain stressful or threatening conditions. (*AR*)

Hassenstein, B. and Reichardt, W. **System theoretical analysis of the time, sequence, and sign evaluation in the perception of motion of the weevil *Chlorophanus* (Systemtheoretische Analyse der Zeit-, Reihenfolgen- und Vorzeichenauswertung bei der Bewegungsperzeption des Rüsselkäfers *Chlorophanus*).** *Zt. f. Naturforschung*, 1956, 11 b, 513–524.

The optokinetic reactions of the weevil *Chlorophanus viridis* were analyzed in terms of input (light stimuli presented from the periphery of a circle with the animal in its center) and output (tendency of the weevil to turn clockwise or counterclockwise); on this basis the authors attempt to build a model system having the same characteristics, i.e., the same "transfer function," as the test animal. The light pencils used were "punctiform": each of them covered the domain of only one ommatidium (eye facet), so that it was possible to vary at will the time and space location of two or more illuminated ommatidia. The experiments are only briefly mentioned since details thereof will be reported in a forthcoming publication (to appear in *Zt. vergleich. Physiol.*)

The authors' general criteria of system analysis are given in a previous paper (*Zt. f. Naturforschung*, 1953, 8 b, 518–524); they emphasize that their aim is *not* to find *a* system having the same performance characteristics as the biological organism, but rather the simplest possible such "analogue system", or, to use their terminology, one which has just the necessary and sufficient attributes. There is, of course, no guarantee that such a simple structure will actually be realized in nature, but the authors believe their method of analysis to be a suitable starting point in all cases.

Three input parameters were found to affect the mode of performance significantly; for brevity they are denoted as (*a*) Time, (*b*) Sequence, and (*c*) Sign. "Time" signifies the time interval, Δt, between two successive stimulations. The authors conclude that the first stimulus, at time t_1, releases a burst of excitation, say ϵ, which after having reached its maximum value becomes a decreasing function of time. The intensity of the second stimulus is multiplied by the value of ϵ (at time $t_1 + \Delta t$) to give the net amount of excitation governing the final reaction. A device which performs this "multiplication" is therefore included in the system, and the value of ϵ at any particular instant constitutes a measure for the time interval Δt (the ascending phase of ϵ is of short duration and biologically unimportant). "Sequence" is determined by whether ommatidium *A* was hit by the first light flash and ommatidium *B* by the second, or vice versa; it indicates the direction of motion in the weevil's environment. "Sign" refers to stimulations of varying intensity. Let a stimulus be called positive (+) if its intensity increases (from "dark" to "bright") and negative (−) in the opposite case. Then, for two successive stimuli, it is found that, other things being equal, the combination [+, +] is equivalent to [−, −] (weevil turns in the direction determined by the time sequence of the two stimuli), whereas either of the combinations [+, −] and [−, +] produces the inverse effect.

All three factors are examined separately, and then combined into a final structure (Figure 8 of the paper under review) which, though still relatively easy to survey, is nevertheless too complicated to be described in a brief summary.

No attempt is made to correlate the structural elements of the model with actual nervous pathways, synapses, ganglia, etc. In the model the individual elements are endowed with the simplest possible properties; it is assumed, for example, that each "transmitting line," or fiber, can send information by modifying the value of only *one* positive parameter. As the authors

point out, if the central nervous system were able to handle the transmission of information by a more efficient code, then the actual biological system could be *structurally* still simpler than the postulated model. This alternative is, however, unlikely. (*ET*)

Mackworth, J. F., & Mackworth, N. H. **The overlapping of signals for decisions.** *Amer. J. Psychol.*, 1956, 69, 26–47.

Two experiments are reported on the effect of reception of information for decisions coming over more than one channel. Measurement of information handling is relatively simple in the single-channel case; average speed of presentation of decision demands seems to account for a large part of the variance in performance. This measure is not adequate in the multi-channel situation since decision demands occur at different rates in the different channels and in many cases the demands for decision will occur simultaneously. The amount of such overlap seems to add a stress factor of a different kind than the simple speed of decision demands.

The experimental situation required that *Ss* watch information arriving on from two to twelve channels. The information was in the form of cards each bearing six items of information in the form of simple geometric symbols about "flight plans." Each channel was a window through which the cards appeared. The problem was for *S* to say how many symbols in the channels were common to a fixed card at any given moment. The cards appeared in different channels at different rates but average speed of arrival was controlled; six different average speeds were tested.

An index of signal-overlap was developed; the total sum in seconds of the various periods during which a given signal is overlapped by any other signal. This index provides a measure of variations in stress on the different channels over time.

Results indicate that both average speed of presentation and the number of channels significantly affect performance. Most of the multi-channel effect can be traced to variation in stress as measured by the index of signal-overlap. A linear relation was demonstrated for the regression of missed signals on signal-overlap. "The greater the peak-stress, the higher the proportion of failure due to missed rather than wrong decisions." (*AH*)

Menger, K. **Why Johnny hates math.** *Math. Teacher*, 1956, 49, 578–584.

Problems of pedagogy are here presented intimately intertwined with those of semantics of mathematics. The author, a mathematician, suggests that in addition to psychological, sociological, and other factors usually cited as underlying the inadequacy of mathematical education in the United States, there are important stumbling blocks in the language of mathematics itself which may be responsible for a growing aversion to this "language" in late childhood and adolescence.

Whereas arithmetic is learned sufficiently early in life so that it is learned as a "native language," i.e., intuitively, idiosyncratic inconsistencies being taken for granted, mathematics on the high school and college level is learned as adults learn a foreign language. Under these circumstances the student needs consistent "rules" (a grammar) and is impatient with bewildering exceptions. In spite of the widespread notion that the language of mathematics is entirely rigorous and unambiguous, these exceptions, ambiguities, and inconsistencies are extremely numerous and, to make matters worse, mathematics teachers and even mathematicians are largely unaware of them (having themselves internalized the language) and so are unable to cope with them pedagogically.

The author cites numerous examples of glaring idiosyncracies of mathematical usage, traces their source to the codification of that usage (around 1700) and suggests that the unclarity of mathematical language tends to divorce in Johnny's mind mathematics from common sense. Consequently Johnny is usually unable to put mathematical techniques to use and even if he does so, he tends to regard mathematics as a sort of magic, in which incantations must be

learned, instead of the epitome of common sense, a powerful intellectual tool.

These difficulties, according to the author, are not experienced by mathematicians (the "virtuosi"), whose intuition guides them around the difficulties, nor, perhaps, by Johnny's cousins abroad who "enjoy intellectual acrobatics" more than Johnny does. Johnny, whether he realizes it or not, thirsts for clarity and does not get it because of the semantic morass pervading contemporary mathematical usage, stemming, as Whitehead once said from "a passionate attachment of some leaders of thought to the cycle of ideas within which they received their own mental stimulus at an impressionable age."

The solution proposed by the author is a revision of mathematical terminology centered primarily in the uses of "variable," "function," "co-ordinates," "indeterminates," etc. Extensive discussion of these matters is found in the author's previous writings. (*AR*)

Simon, Herbert A. **A comparison of game theory and learning theory.** *Psychometrika*, 1956, 21, 267–272.

Given a repeated choice between two alternatives, both receiving partial reinforcement at constant but different rates, naive subjects reach asymptotic values below 1 in their preference for the alternative with higher reward frequency. This behavior is in accordance with predictions generated by Estes' learning model. From the point of view of game theory, however, this distribution of choices seems irrational since a pure strategy of persistently choosing the more frequently rewarded alternative would maximize the payoff.

On the basis of arguments first advanced by M. W. Flood, Simon shows that the behavior in question can still be considered rational if certain plausible assumptions are made. According to these assumptions, the subject (1) does not realize that the reward ratios for the two alternatives are constant, and (2) does not try to maximize the expected payoff but is minimaxing his *regret* (i.e., the difference between the reward obtainable by a given strategy and the reward possible could he outsmart the experimenter). In other words, the subject may assume that a strategy of persistent preference for the "better" alternative will result in a constant probability of reward smaller than 1, and also make it impossible to discover, by a strategy of switching choices, a presumably existing pattern of reward with maximal payoff. Under these conditions the experimentally obtained behavior of switching with probabilities corresponding to the reward ratios would be the best strategy. Formal derivation of these conclusions is given.

It may be difficult to decide whether subjects actually are minimaxing regret or are simply behaving adaptively on the basis of some learning mechanism. But the important point of the argument is the indication, that it may be necessary to distinguish clearly between *objective* and *subjective* rationality, the latter based on the frame of reference of the subject who may have incomplete information or may be maximizing something else than what the game theorist assumes. Although this distinction seems to raise enormous problems for the practical use of the postulate of rationality, the concept does not become entirely useless. This is indicated by the fact that persons familiar with game theory will, in the given experiment, learn to adopt a strategy of persistent choice, presumably not because they learn differently, but because they come to adopt a perception of the situation parallel to the assumptions made in the game theoretical analysis. (*FS*)

About the Authors

Theodore L. Clemens, co-author of "Studies of Autonomic Response Patterns: Rationale and Methods," is Research Psychologist at the University of California at Los Angeles. His present research is concerned with the evaluation of the role of autonomic nervous system functions in psychosomatic, organic, and psychological disorders, and he has written several articles on this problem.

Robert A. Dahl ("The Concept of Power") holds the Eugene Meyer Professorship of Political Science at Yale University. Over the next year or so he will be engaged in a study of the structure of power in New Haven, during which study he hopes to uncover a good deal about the individual and social factors associated with "power" in New Haven. He comments that a political scientist can hardly earn his keep without using the term "power," and that he felt increasingly frustrated trying to hold on to such a slippery term. With his graduate students he had tried on a number of occasions to explore it further, but "I didn't have any sense of progress until last year at the Center for Advanced Study in the Behavioral Sciences, where James March (who is at Carnegie Tech) and I organized ourselves into an informal two-man team to go to work on it. . . . Having, as I thought, pretty well clarified the term in my own mind, I hadn't intended to write a paper on it. But after giving the essential argument in a graduate seminar on the applications of mathematics to the social sciences, I succumbed to the usual temptation."

Bernard T. Engel, co-author of "Studies of Autonomic Response Patterns: Rationale and Methods," received his Ph.D. from the University of California at Los Angeles, where he is at present Research Psychologist. He is interested in basic research on the problem of autonomic nervous system activity, particularly on the interaction between basal level and intensity of response.

Jum Nunnally ("The Communication of Mental Health Information: A Comparison of the Opinions of Experts and the Public with Mass Media Presentations") is currently Assistant Professor in the Department of Psychology and Institute of Communications Research at the University of Illinois. His interest is jointly in psychology, in measurement methods, statistics, and experimental design, and in communications, involving an effort to adapt psychological measurement methods and experimental techniques to problems in human communications. The present paper is one report from a larger project being undertaken by an interdisciplinary group in the Institute of Communications Research. "We have undertaken a long range research program to determine how best to use communications media to augment public understanding about mental health matters. . . . Our subsequent research is concerned with an exploration of the 'dynamics' of beliefs. Some current researches are (1) small group studies, (2) studies of the language structure of mental health beliefs, (3) studies of attitude change, (4) studies of public interest in different mental health topics, (5) studies of the incentive value of differently structured messages, and (6) studies of the differential effectiveness of message stylings."

Claire Russell ("An Approach to Human Ethology") was born and educated in Berlin. After the advent of the Nazis, she continued her education in Paris and later in Oxford, where her studies included experimental psychology and training in psychoanalysis. She was interested in developing a satisfactory general approach to the study of human behavior, especially the problem of releasing creative potentialities. She employed Freud's techniques and initially his theories in her clinical work, but was forced by her own observations to modify the Freudian techniques, and radically to overhaul the theory. Her clinical work has led her to form a number of powerful heuristic hypotheses about the treatment of children and its effects on the development of their behavior, based, however, on indirect observation with exclusively adult patients; many of these hypotheses are now being directly verified.

W. M. S. Russell ("An Approach to Human Ethology"), born in Plymouth, England, is currently Hon. Research Assistant in the Department of Zoology and Comparative Anatomy at University College London. The record of his education and training reveals that he has always been interested in human behavior, but from many angles. While at Oxford he engaged in

experimental work on clawed frogs, his main interests then being the relations between endocrinology and behavior, and the properties of instinctive mechanisms in lower animals. His own experiments were forcing him to take an interest in the methodology of research in general and behavior study in particular. Close collaboration with workers in other related fields stimulated the present article, which has been expanded into a book to be published by Blackwells Scientific Publications.

The Russells say they hope very much that "the publication of this article will lead to stimulating suggestions and criticisms, for we are well aware that systematic study of human behaviour is still, like Homo sapiens himself, in its evolutionary infancy."

Marion A. Wenger, co-author of "Studies of Autonomic Response Patterns: Rationale and Methods," is Professor of Psychology at the University of California at Los Angeles. He states that the present article is an outgrowth of twenty years' study of autonomic functions in children and adults. "The results of these studies indicated that fruitful information regarding emotional behavior can be obtained with multiple ANS recording under a variety of stimulus conditions." Wenger's publications reflect his research interests, which center around the evaluation of the role of autonomic activity in human behavior, particularly as it relates to emotional behavior. In addition, he is co-author of the recently published book *Physiological Psychology.*

ANNOUNCEMENT

Nobody Knows: A Survey of Unsolved Problems and Forgotten Facts

All well-informed persons realize that unknown truths are probably as numerous as the known ones. But nobody has yet attempted to explore in detail what remains to be done in each and every area of knowledge. That is what we propose to do.

Consequently, if readers of this journal are aware of any significant problem in their own field which has up till now remained unsolved, or any important fact which has been somehow neglected, they are invited to take part in our survey, entitled as above, which will be published in 1958 by the Philosophical Library in N.Y.C.

This survey may occupy several volumes. Containing a great variety of material arranged by topics and provided with cross references, it will serve as a reference work for scholars and students. We mention as typical entries: that nobody knows the causes of cancer, that nobody has explained the physiology of sleep, that nobody has succeeded in proving that Joan of Arc was actually burned to death.

All papers should be limited to a mere exposition of the problem in question and may not exceed 1,500 words. Each author is asked to write so as to be understood by the general reader. Papers are expected to be properly typewritten, dated and signed; and, if the writer so wishes, the name of the institution he is connected with may appear under his signature. A few bibliographical references are desirable.

Our Board of Editors, composed of recognized authorities in each field, will read every article submitted, and each author will receive full credit for every published piece.

Papers should be sent to Dr. Ralph B. Winn, 224 Cummings Ave., Elberon, N. J.

ROBERT E. PIKE, Ph. D.
RALPH B. WINN, Ph.D.

ANTHROPOLOGY AS A COMPARATIVE SCIENCE

by George Peter Murdock

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There is a story, perhaps apocryphal, of the three tribal chiefs who were asked what their people did with their fathers when they died. One chief replied, "We eat them." Another said, "We burn them." And the third said, "We bury them." Each chief was unbelievably shocked by the repulsive, barbarous, and sacrilegious practices of the other two. Anthropology as a comparative science of man seeks, along with sociology and psychology, systematically to study and to understand the many different kinds of behavior found in the thousands of human societies known to history and ethnography, for only with such a cross-cultural approach will it be possible to create a science of human behavior.

ANTHROPOLOGY, in common with its sister disciplines in the behavioral sciences, has at least three major objectives. The first of these, which is pursued principally by archeology and is shared especially with history, is to reconstruct the record of the origin, spread, differentiation, and evolution of culture from its simplest beginnings to the manifold forms that occur throughout the world in modern times. A second major objective, which anthropological students of primitive art, music, folklore, religion, and philosophy share with the various fields of the humanities, is to attain an understanding and appreciation of the diverse manifestations of the human spirit. The third ultimate goal is to create a science of human behavior by developing a systematic body of verified knowledge about the principles governing the interaction of men in society, the formation of groups and of social norms of behavior, and the transmission and differentiation of culture. It is with this third objective, shared by anthropology especially with sociology and psychology, that this paper is concerned.

There are many approaches to the achievement of a science of human behavior, but attention shall be confined here to one of them, the comparative or cross-cultural approach. This approach is not uniquely anthropological, for it has also been diligently pursued by sociologists and has been employed to a more limited extent by psychologists, historians, and others.

The first point to be made is that there can never be any generally valid science of man which is not specifically adapted to, and tested with reference to, the diverse manifestations of human behavior encountered in the thousands of human societies differing from our own that are known to history and ethnography. Whatever other methods of investigation may be employed—and they are numerous—the comparative method is indispensable. Without it, no combination of other methods can achieve scientific results of universal application. At the most they can only produce culture-bound generalizations, approximately valid for a particular group of related societies during a particular segment of their history, but incapable of generalization to other societies except as highly tentative working hypotheses, and equally incapable of predicting future developments in periods of rapid social change or even of comprehending them after they have occurred.

The admirable body of principles worked out in our sister science of economics provides an excellent illustration. Originally adapted to a group of European societies which had passed through the Industrial

Revolution and were characterized by individual enterprise, a money economy, and mechanisms of exchange and distribution relatively unfettered by governmental intervention, they have had to be revised radically to fit even approximately the systems of distribution and price determination found in some of the same societies after they had adopted fascist, communist, and socialist governments with rigorous state controls. Even thus modified, the principles of economics still provide the anthropologist with remarkably little guidance for the understanding of economic behavior in a host of simpler societies.

Political science is also culture bound, though its limits are somewhat less restricted, extending back in time well before the Industrial Revolution to the period of Plato and Aristotle and stretching in space to include the major societies of Asia as well as of Europe. Historical research normally encompasses very much narrower ranges of time and space. Its conclusions are so invariably specific to a single culture at a particular period of time that of course, in this sense, "history never repeats itself." The great body of research in sociology, being conducted in our own society or in closely related ones in the European tradition, is also culture bound, and even its best validated conclusions cannot be accepted as universally valid until they have been cross-culturally tested and verified. Unlike economists, political scientists, and historians, however, the leading sociologists of every generation have been aware of this fact and have deliberately acquired substantial conversance with ethnography with the aim of achieving universality for their generalizations. It is necessary only to cite such names as Herbert Spencer, Emile Durkheim, William Graham Sumner, W. I. Thomas, A. G. Keller, and Talcott Parsons.

From at least the time of Wundt an occasional psychologist has manifested an interest in the cross-cultural approach, and this awareness appears to be currently on the increase, as witness the recent research activities of Irvin Child and Leonard Doob at Yale on socialization and cultural change respectively. On the whole, however, psychologists have tended to concentrate upon unraveling the basic mechanisms of behavior, whether of perception, learning, or personality development, and when they have attempted to explain cultural phenomena have usually applied their principles directly without reference to the highly diverse conditions under which these operate in different societies. From Freud's *Totem and Taboo* to the present, consequently, most psychological interpretations of cultural phenomena have erred in seeking to explain too much, in contrast to sociological interpretations, which typically explain much too little.

Merely to assert the importance and the indispensability of a comparative approach to the study of human behavior is, of course, not enough. Allegations should be buttressed by at least some measure of empirical support. This is now presented in the form of a simple quantitative demonstration based on a survey of representative cultures from all regions of the world and from different periods of history compiled independently for quite another purpose.

In this survey, each society was classified with respect to thirty distinct cultural categories, mainly in the areas of productive economy, division of labor, settlement patterns, government, social stratification, marriage, and kinship. This classification was examined to determine the extent to which cultures of European derivation resemble, or differ from, both one another and cultures of other regions. Therefore a purely random selection was made of ten societies each from Asia, Africa, Oceania, and native North and South America. Ten European societies also were selected—not at random, for this might have led to the inclusion of closely related peoples, but in a manner consciously intended to obtain the widest possible range in regard to time, space, language, and cultural diversity. These European societies were the following:

The Athenians of the Periclean Age, about 450 B.C.;

The Boers of the Transvaal after the Great Trek, about 1850 A.D.;

The Brazilians of Bahia about 1650 A.D.;

The English of the Elizabethan period, about 1600 A.D.;
The Hungarians immediately prior to World War II;
The Icelanders of about 1100 A.D.;
The Lithuanians prior to their absorption by Soviet Russia;
The Romans of the Imperial period, about 100 A.D.;
The Russians of the Soviet Union today;
The Spaniards of modern Andalusia.

When the classification of our thirty cultural items for the contemporary United States was compared with those for these diverse European societies, 61 per cent of all items were discovered to be identical and only 39 per cent variant. But when they were compared with those for other regions of the world, the identities fell to 26 per cent, ranging from 30 per cent for Asia to 22 per cent for Africa. Other tests confirmed the probability that any two distantly related European cultures will share about six out of ten classificatory elements, whereas only about one in four will be similar when any European culture is compared with any non-European culture. This suggests how unsafe it is for a sociologist, psychologist, or historian to generalize his knowledge of Euro-American societies, however profound, to mankind in general.

Moreover, almost 50 per cent of the items listed for the 50 randomly selected non-European societies fell into categories which were completely unrepresented in any of the European cultures, suggesting that a complete analysis of all European societies would reveal only about half of the major classificatory categories under which anthropologists customarily analyze the cultures of the world. Note that the reference here is to purely classificatory categories, not to specific items of cultural content. For these the differences between European and non-European cultures would be infinitely more numerous.

Customs of other peoples for which there is no precedent in European cultures, and which even lie quite beyond the range of human behavioral variation conceivable to most Europeans, are a stock-in-trade of anthropologists, and constitute one of the reasons why this profession is such a detestable bunch of bubble-prickers. Two examples selected from personal field experience on the island of Truk in the central Pacific are presented.

The normal, routine posture assumed by the Trukese in copulation is one that is not even mentioned in Kinsey or, to my knowledge, in any other occidental work on sexology or pornography. It probably has never even been approximated by any native-born American couple, however experimental. The curious may be referred to Malinowski, who describes a similar posture for the Trobrianders.

If the Trukese fall outside the range of European expectations in an aspect of behavior so closely linked to instinctive reflexes, comparable divergences in purely ideational behavior are certainly to be expected. Thus the people of Truk have a concept and expression, "kinisou," which expresses a sense of personal indebtedness, and corresponds to the English phrase, "Thank you," for which it is, indeed, the customary term. In addition to expressing obligation for a gift or favor received, however, "kinisou" is used to verbalize the attitude of a victim of sorcery toward the person who has employed the black arts to his detriment. To us, these two situations appear to have nothing in common, but to the Trukese they seem nearly identical. The recipient of a gift or favor has incurred an obligation which he is bound to repay. Sorcery, to the Trukese, is inconceivable except as a reaction to some unfulfilled social obligation. Hence the victim of sorcery, by saying "kinisou," acknowledges his obligation, his gratitude for having it brought to his attention, and his intention of fulfilling it. When he does this, the sorcerizing ceases and social equilibrium is restored.

Perhaps I have dwelt over-long on the indispensability of a cross-cultural approach to the social scientist if he is to comprehend the range of variation actually exhibited in human social behavior and arrive at generalizations valid throughout this range. Some

of the methods and problems of comparative research also deserve attention.

Throughout the nineteenth century, and in some instances well into the twentieth, the comparative method, whether used by anthropologists, sociologists, psychologists, or jurisprudes, consisted essentially in surveying a wide range of ethnographic sources and extracting masses of cases in support of some hypothesis, evolutionary or otherwise, which had been formulated beforehand on other grounds. In the hands of scrupulous scholars this often brought considerable illumination, but the procedure had two serious defects. First, it lent itself too readily to perversion, for the treasure-house of ethnography is so rich and varied that it is relatively easy to support almost any hypothesis by a conscious or unconscious selection of cases. Second, the cases themselves were torn from their context, obliterating their relationships with other aspects of the cultures in which they were embedded. It was Tylor (9), in 1889, who first recognized that these intra-cultural relationships, or "adhesions" as he called them, may be considerably more significant than the isolated elements of culture themselves. In recent decades all users of the comparative method accept Tylor's position, refrain from the mere amassing of isolated case material, and adopt some form of what Eggan (1) has called "the method of controlled comparison."

One kind of controlled comparison is to utilize ethnography as the equivalent of a laboratory—as an array, so to speak, of nature-made experiments. A scientific problem is subjected to test by close examination of some society where the relevant variables happen to be assembled in precisely the combination one would bring together if one were able to construct an experimental situation. This particular technique has been employed with success by Mead (6), using Samoa as a crucial case for the study of adolescent problems, and by Holmberg (3), using the Siriono of Bolivia to test his hypothesis regarding the relationship of drive deprivation to the content of dreams and other manifestations of the unconscious.

When a scientific hypothesis relates to items of cultural behavior that are of relatively rare occurrence but are nevertheless historically independent, one may attempt to assemble all the known ethnographic examples and test the hypothesis by analyzing the "adhesions" or cultural associations in which they are embedded. This has recently been done, for example, by Homans and Schneider (4) in an effort to account for the two different types of preferential asymmetrical cross-cousin marriage.

A danger of this procedure is that the discovered associations may occur and have other correlates in the societies rejected because they do not exhibit the phenomenon under examination, thereby possibly invalidating the presumed causal relationship. Tylor's remedy was to examine a large number of cultures drawn from the entire range of ethnography, regardless of the presence or absence of the particular phenomenon under investigation, and to determine its degree of correlation with the presumably associated variables by statistical means. He has been followed by many subsequent users of the comparative method, e.g., Hobhouse, Wheeler, and Ginsberg (2), Murdock (7), Simmons (8), Horton (5), Beatrice Whiting (10), and Whiting and Child (11).

The great problem in worldwide statistical comparisons is the selection of the sample. It would be improper to include the entire universe of known cultures, even if this were feasible, for cultures are not discrete and completely independent units. Complete coverage would result in the heavy over-representation of areas with many small and independent societies of similar culture, such as the Basin Shoshoneans, and in the under-representation of large and integrated societies, like those of China and Western Europe, and of small and isolated societies with distinctive ways of life, like the Tasmanians, the Vedda of Ceylon, and the Guanche of the Canary Islands.

Nor can the cultures compared be chosen by any method of random sampling. Not only would this result equally in the disproportionate representation of small, large, and unique cultures, but in addition it would mean the selection of many cultures

that are inadequately described and the rejection of many of those for which the literature provides the fullest and most pertinent information.

Nor can we be content merely with choosing a large number of cases for which good descriptive information is available. Hobhouse, Wheeler, and Ginsberg did this, with the result that a substantial majority of their hunting and gathering cultures were selected from aboriginal Australia. Consequently all their correlations of other traits with hunting and gathering were meaningless except perhaps as crude reflections of native Australian culture.

In my *Social Structure* (7), I made what now seems to me a half-hearted attempt to combine the requirements of good descriptive coverage, wide geographical distribution, and restriction of the number of cases selected from any particular culture area. Errors were made in including some cases that were essentially duplicates of one another, and in not striving hard enough to secure cases from areas like eastern Indonesia, Indochina, Central Asia, Europe, North Africa, and Middle and South America where the sources were relatively inaccessible to me for linguistic or other reasons. This was demonstrated two years ago in a graduate class where we assembled two worldwide samples of 300 societies each, one completely unselected and the other very carefully compiled to give equal representation to all the culture areas of the world. The major statistical correlations of the *Social Structure* volume were recalculated for these two samples. While there were no serious upsets, the results from the selected and unselected samples were sufficiently divergent in enough instances to convince me of the imperative need of far more careful representative sampling.

I have since been engaged in working out a world sample of some 550 cultures, with tabulated data on basic economy and social and political organization. This will cover the complex civilizations of Europe and Asia, and those recorded from antiquity, on exactly the same basis as preliterate cultures. It will give, as nearly as possible, equal numerical representation to all the major variants in man's way of life, and will include, for each culture area, a number of cases closely proportionate to its diversity within the entire range of known cultures and carefully distributed by sub-areas.

The historical relatedness of different cultures through diffusion does not cause me the concern that others seem to exhibit. The evidence is now fairly clear that societies borrow from one another, much as they invent for themselves, elements for which they feel a need and which are reasonably congruent with existing usages, and that borrowed like invented and pre-existing traits undergo a process of integrative modification. However, a world sample must not include cultures so similar as to constitute genuine duplication. Provided care is taken to guard against this, as by avoiding the inclusion of geographically contiguous societies unless they differ significantly in language, economy, or social organization, similarities in culture, whether due to diffusion or to parallel adaptation, can be assumed to have undergone integration with their respective cultural contexts and can therefore usually be safely treated as essentially independent.

Though this paper is entitled "Anthropology as a Comparative Science," sociologists have been at least as prolific in comparative studies as have anthropologists, and there is no reason why the method cannot be used equally well by psychologists or any other group of scientific students of human behavior. The only block is the lack of familiarity with, and accessibility to, the ethnographic sources. And this the Human Relations Area Files is specifically designed to overcome.

In conclusion, I may perhaps be forgiven for recounting a true story as a sort of parable of the need of a cross-cultural orientation. When a ruling Arab sheikh recently visited a certain American city, the matron who was the leader of local society gave a ceremonial dinner in his honor. When he arrived with his retinue, they were offered cocktails. Being a Moslem, the sheikh felt compelled to observe his religion's taboo on alcoholic beverages, at least on so public an occasion, and intimated that he would accept

a Coca Cola instead. There happened to be none in the house, so the butler was dispatched to the neighborhood store. While he was away, the others present courteously but uncomfortably refrained from drinking out of respect to the guest of honor. Later at the dinner table, when the entree was brought on, it was, of all things, a roast suckling pig with an apple in its mouth. When the Arabs politely refused servings of this abomination, the embarrassed hostess had nothing to offer as a substitute except cold cuts. And it was soon discovered that the only cold cuts in the house were ham.

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Mankind is now in one of its rare moods of shifting its outlook. The mere compulsion of tradition has lost its force. It is the business of philosophers, students, and practical men to re-create and re-enact a vision of the world, conservative and radical, including those elements of reverence and order without which society lapses into riot, a vision penetrated through and through with unflinching rationality. Such a vision is the knowledge which Plato identified with virtue.

Epochs for which, within the limits of their development, this vision has been widespread, are the epochs unfading in the memory of mankind. There is now no choice before us: either we must succeed in providing a rational co-ordination of impulses and thoughts, or for centuries civilisation will sink into a mere welter of minor excitements. We must produce a great age, or see the collapse of the upward striving of our race.

—A. N. WHITEHEAD

STRUCTURAL DUALITY

by Frank Harary[1]

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Charles L. Dodgson, the eminent Oxford mathematician, once expounded, in the form of a conversation between an egg and a seven-and-a-half-year-old girl, the interesting concept of an "unbirthday." Conversely and perhaps complementarily, lexiphiles enjoy making lists of words like couth, plussed, ept, ebriety, peccable. Structural duality is, of course, more than whimsical playing with words, but it is concerned with such notions as contrastatus (versus status), antibalance (versus balance), and unliaison person (versus liaison person). It also involves the use of various kinds of graphs as a mathematical model, an approach whose value lies in the fact that it is not bound by physical units of measurement. The rigorous mathematical development of a duality theory may provide a source of fruitful concepts for behavioral science.

OUR purpose is to develop, formalize, and discuss three basic laws for structural duality. In so doing, we hope to provide a unifying framework for several concepts which have already been discussed either explicitly or implicitly in the literature of the behavioral sciences. In addition we attempt to create a systematic theory which has the philosophical purpose of extending our conceptual resources rather than the relatively materialistic motive of providing a specific model for a specific setting. Intuitively the three laws involve interchanging the relational motions of presence and absence, forward and backward, and positive and negative. The present paper is a sequel to three earlier papers (13, 14, 6) in which there appear introductory expositions of the theory of graphs, directed graphs, and signed graphs respectively. The operations of complementation, conversion, and negation on these three types of graphs lead to the three laws for structural duality.

These notions of duality are not new. For example, we were anticipated by Freud in 1915, who wrote:

> Loving admits of not merely one, but of three antitheses. First there is the antithesis of loving-hating; secondly there is loving-being loved; and, in addition to these, loving and hating together are the opposite of the condition of neutrality or indifference (9, p. 76).

It will be seen that these three antitheses of Freud correspond respectively to our "antithetical," "directional," and "existential" duality laws.

In developing the laws for structural duality various kinds of graphs are used. A *graph* (13, 17) is a finite collection of *points* $A, B, C, \ldots$ together with a prescribed set of *unordered* pairs of distinct points, called *lines*. A *directed graph* or *digraph* (13, 17) also consists of a finite collection of *points*, but its *lines* are *ordered* pairs of distinct points. A *signed graph* (6, 11) is obtained from a graph by taking some of its lines as *negative* and the remaining lines as *positive*.

Graphs and directed graphs have been used as a mathematical model in various applications of communication theory to group problem solving by Bavelas *et al.* in the series

[1] The author wishes to thank D. Cartwright, C. H. Hubbell, and I. C. Ross for some stimulating and helpful discussions.

of papers (3, 15, 18). In addition Rapoport (20) has used graphs implicitly in his theorizing on pecking sequences. More recently, Ashby (1) has considered directed graphs under the name of "kinematic graphs" in introducing the notions of cybernetics. In the field of organization theory, Weiss *et al.* (16, 22, 29) have made use of graphs and directed graphs as models for the structure of an organization. We shall see that these results either implicitly allude to or may be enriched by the appropriate use of the laws of structural duality.

We begin with examples of duality taken from mathematics: set theory, logic, and group theory; and from physics: future-past, right-left, and interchanging the sign of charged particles. On turning to structural duality, we shall find that some new concepts are induced and we shall explore some of these, such as unliaison person, contrastatus, and antibalance. We then derive two further duality laws from the three basic laws, and show that no additional laws are derivable from these.

DUALITY

In general, any duality principle has the two properties: (1) the dual of the dual of a statement is the original statement, and (2) the dual of a true statement is true. A classic example of duality occurs in set theory (31, Chap. III), interchanging union and intersection, or equivalently in propositional logic (31, Chap. IX), interchanging disjunction and conjunction, i.e., the connectives "or" and "and." Thus the dual of the logical DeMorgan Law:

$$\text{not } (p \text{ or } q) = (\text{not } p) \text{ and } (\text{not } q) \quad [1]$$

is

$$\text{not } (p \text{ and } q) = (\text{not } p) \text{ or } (\text{not } q). \quad [2]$$

Similarly the dual of the commutative law for set-addition:[2]

$$A \cup B = B \cup A \quad [3]$$

is the commutative law for set-multiplication:

$$A \cap B = B \cap A. \quad [4]$$

A *self-dual* statement is one whose dual is the same statement. An example of a self-dual logical law is that of double negation:

$$\text{not } (\text{not } p) = p. \quad [5]$$

This law is self-dual since there are no "and" or "or" connectives to interchange. Another self-dual statement, taken from set theory, is given by the equation:

$$A \cap (A \cup B) = A \cup (A \cap B) \quad [6]$$

sometimes called the "law of absorption," since both expressions are equal to the set A. On interchanging the operations union and intersection each side of this equation is transformed into the other side.

In view of the DeMorgan laws, the interchange of the operations union and intersection can be applied by taking the complement or negation of both sides of a set theoretic or propositional equation, respectively. The law of double negation, which can be written for sets in the form:

$$(A')' = A, \quad [7]$$

then assures us that this process is a dual one.

In his celebrated paper (24) which introduced modern switching theory, Shannon demonstrated the isomorphism between switching functions on two-terminal networks and Boolean functions. In this model, the above logical and set theoretic dualities are realized by an interchange of series connections with parallel connections.

A duality theorem for (mathematical) groups (30, Chap. VII) is given by the transformation which interchanges the order of the terms in each group-product $a \cdot b$. In accordance with this notion, the dual of the left-cancellation law:

$$a \cdot b = a \cdot c \text{ implies } b = c \quad [8]$$

is the right-cancellation law:

$$b \cdot a = c \cdot a \text{ implies } b = c. \quad [9]$$

In the study of the calculus of binary relations by the logicians Tarski and McKinsey,

[2] The *union* of two sets A and B, written $A \cup B$, is that set whose elements lie in at least one of the sets A or B. Their *intersection*, $A \cap B$, is the set of all elements which are in both A and B. The *complement* A' of A is the set of all elements under consideration which are not in A.

several kinds of duality occur. These include complementary duality, converse duality, and the combination of these, which correspond precisely to our existential, directional, and (the combined) direxistential duality. It is these relational dualities which are mentioned in the following quotation from the beginning of an article by Gottschalk (10).

> It is well-known that every involution in a logical or mathematical system gives rise to a theory of duality; for example, negation in the sentential calculus and predicate calculus, complementation in the calculus of classes, complementation and conversion in the calculus of relations, etc. The purpose of this note is to call attention to the fact that every involution in a logical or mathematical system gives rise to a theory of *quarternality*. . . .

Gottschalk then describes the pairs of dual "constants": {*true*, *false*}, {*and*, *or*}, {implies, is not implied by}, {is implied by, does not imply}, {equivalent, inequivalent}, and an apparently unintentional pun {*stroke* and *dagger*}, where *a stroke b* means not-*a* and not-*b*, while *a dagger b* stands for not-*a* or not-*b*. The "constant" *not* is self-dual. His quaternality theorem for logic is that a formula yields three others on negating the "variables," "constants," or both.

There is a beautifully written exposition of three physical duality laws by Uhlenbeck (28), one of which is also discussed in a similar vein by Blatt (5). The first of these duality laws can be called the future-past law, or time reversal, or *temporal duality*. This law asserts that for each physical law involving time, the interchange of positive or future time with negative or past time results in another (possibly the same) law. A purely mathematical description of basically the same phenomenon is given by Feller (7) in his statement of the "backward equations" for Markov chains.

The second of these physical duality laws is the right-left law or *directional duality*. In accordance with this law, replacement of the distance abscissa $+x$ in a physical law by the variable $-x$ results in the same physical law. That is, the designation of whether right or left is to be called the positive position of the x-axis is physically immaterial.

The third physical duality law is that of *charge duality*, which proposes that to each charged particle, there corresponds another charged particle of the same mass but of opposite charge. It is this duality principle which predicted the existence of a positive electron or "positron," and of a negative proton or "antiproton" even before these two particles were experimentally observed. Thus these three physical duality laws have to do with changing the sign of time, distance, and charge respectively.

STRUCTURAL DUALITY

I. The first kind of structural duality is based on the operation of taking the complement of a graph or of a digraph. The *complement* of a graph $\mathcal{G}$ is that graph $\mathcal{G}'$ having the same set of points as $\mathcal{G}$, but in which two points are adjacent, or joined by a line, if and only if they are not adjacent in $\mathcal{G}$. Thus the lines that are present in $\mathcal{G}'$ are precisely those which are absent in $\mathcal{G}$. For example the complements of the graphs $\mathcal{G}_1$ and $\mathcal{G}_2$ in the top half of Figure 1 are the graphs $\mathcal{G}_1'$ and $\mathcal{G}_2'$ in the lower half. We note that the graph $\mathcal{G}_2$ is *self-complementary* in the sense that $\mathcal{G}_2'$ is isomorphic to $\mathcal{G}_2$. Also $\mathcal{G}_1'$ is a disconnected graph even though $\mathcal{G}_1$ is connected.

By the complementary concept of a concept about a graph $\mathcal{G}$, we mean the same concept for the graph $\mathcal{G}'$.

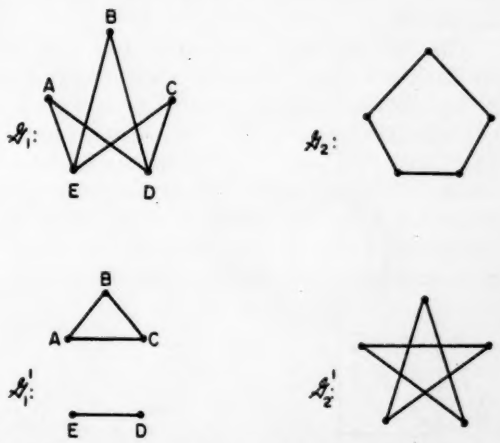

FIG. 1. Examples of two graphs ($\mathcal{G}_1$ and $\mathcal{G}_2$) and their complements ($\mathcal{G}_1'$ and $\mathcal{G}_2'$).

Existential duality

Each theorem about graphs yields another theorem when every concept in the original theorem is replaced by its complementary concept.

We note that the *complementary digraph* $\mathfrak{D}'$ to a digraph $\mathfrak{D}$ is defined by the same presence-absence consideration as for graphs. The only difference is that the lines of a digraph are *directed* from one point to another. Thus Figure 2 shows a digraph $\mathfrak{D}$ and its complement $\mathfrak{D}'$.

We now illustrate the idea of existential duality with the concepts dual to those of an isolated point of a graph, and an articulation point of a graph or equivalently a liaison person in an organization (16, 22, 29).

An *isolated point* of $\mathcal{G}$ is one which is not adjacent to any other points of $\mathcal{G}$. An *unisolated point of* $\mathcal{G}$ is an isolated point of $\mathcal{G}'$. An *articulation point* of a connected graph $\mathcal{G}$ is one whose removal results in a disconnected graph. An *unarticulation point* of $\mathcal{G}$ is an articulation point of $\mathcal{G}'$.

The *degree* of a point of a graph is the number of lines to which it is incident. If p is the number of points in a graph, it is clear that an unisolated point is one whose degree is $p - 1$.

Examples of articulation points are given by a liaison person in an organization (29), an overseas telephone operation (13), or an interpreter such as a unique bilingual person in a group whose remaining members are each monolingual and speak two different languages.

This last situation is used by Bavelas (2) to illustrate inner and outer members of a group. Similarly an isolated point of a graph corresponds to an isolate or isolated person in a group, e.g., one who is not in communication with any other group members. On the other hand the existential opposite, an unisolated point, is realized by a person who is in complete communication with the members of his group. Obviously any group with an unisolated person can have no isolate.

FIG. 2. A digraph and its complement.

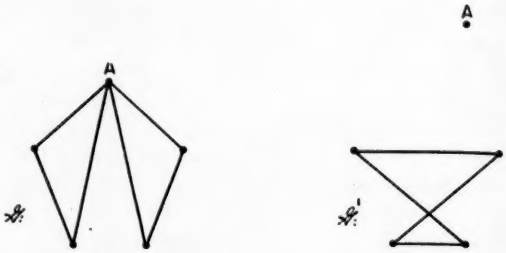

FIG. 3. Graphs showing an unisolated point and an isolated point.

The graphs $\mathcal{G}$ and $\mathcal{G}'$ of Figure 3 serve to illustrate both of these complementary concepts. For, the point A is both an unisolated point of $\mathcal{G}$ and an unarticulation point of $\mathcal{G}'$. A discussion of the existential duals of the theorems concerning graphs and digraphs is beyond the scope of the present article. It would not be entirely unexpected if existential duality should prove to be a fertile field for science fiction (as well as for science itself).

Since none of the concepts of existential duality have been explored empirically, it is not feasible to discuss actual applications. However a well-known anecdote uses these concepts implicitly.

A man who had been shipwrecked on an uncharted island in the Pacific was found there ten years later, leading a Robinson Crusoe sort of existence. His rescuers found that he had built not only a comfortable home, but *two* churches. He explained that the first church was the one he attended while the second was the one he didn't attend.

A variation of the above description of existential duality is implied by the quotation from Freud on the first page of this article. In accordance with his third antithesis, it is ambivalence and indifference which constitute a pair of opposite relations rather than loving and indifference. This is represented graphically by interchanging the presence of both a positive and a negative line joining the same pair of points of a signed graph with the absence of any line between this point-pair.

II. The second kind of structural duality stems from the operation of finding the converse of a digraph. The *converse* $\mathfrak{D}^{\smile}$ of the

FIG. 4. Example of the converse of a digraph (see also Fig. 2).

digraph $\mathfrak{D}$ is that digraph with the same points as $\mathfrak{D}$, in which the (directed) line $\overrightarrow{AB}$ occurs if and only if the line $\overrightarrow{BA}$ is in $\mathfrak{D}$. Thus the converse $\mathfrak{D}^{\smile}$ of the digraph $\mathfrak{D}$ of Figure 2 is given in Figure 4. In addition, Figure 4 shows the converse of the digraph $\mathfrak{D}'$ of Figure 2. We denote the converse of $\mathfrak{D}'$ by $\mathfrak{D}'^{\smile}$. Similarly, we write $\mathfrak{D}^{\smile\prime}$ for the complement of the converse of $\mathfrak{D}$. One readily verifies the general theorem that the unary operations of converse and complement commute with each other, that is:

$$\mathfrak{D}'^{\smile} = \mathfrak{D}^{\smile\prime} \qquad [10]$$

for this digraph $\mathfrak{D}$ by referring to Figure 4.

Directional duality

To each theorem about digraphs, there is a corresponding theorem obtained on replacing every concept by its converse concept.

For example, a *point basis* of a digraph $\mathfrak{D}$ is a minimal collection of points from which all points are reachable. Dually, a point *contrabasis* of $\mathfrak{D}$ is a minimal collection of points of $\mathfrak{D}$ such that at least one of these points is reachable from any point of $\mathfrak{D}$, or equivalently, is a point basis of $\mathfrak{D}^{\smile}$. Thus, the digraph of Figure 5 has A, D as a point basis and B, E as a point contrabasis.

As an example of the use in the literature of a concept equivalent to that of a point contrabasis, we mention Ashby's idea of a "basin" (1). To do this in the terminology of digraph theory, we require some definitions. A *path* from A to E in a digraph is a collection of lines of the form $\overrightarrow{AB}$, $\overrightarrow{BC}$, $\cdots$, $\overrightarrow{DE}$ where the points $A, B, C, \cdots, D, E$ are distinct. The *distance* from A to E is the minimum of the lengths of all paths from A to E; the *length* of a path being the number of lines in it. A digraph is *strong* or *strongly connected* if for every pair of distinct points A, E there is a path from A to E and a path from E to A. A strong component of a digraph is a maximal strong subgraph. The *condensed graph* $C(\mathfrak{D})$ of a digraph $\mathfrak{D}$ is that digraph obtained from $\mathfrak{D}$ on replacing each strong component of $\mathfrak{D}$ by a point and preserving the sense of the directed lines otherwise. A *receiver* of a digraph is a point P such that there are no lines from P to any other point. Then a *basin* [cf. Ashby (1)] is a strong component of $\mathfrak{D}$ which corresponds to a receiver in the condensed graph $C(\mathfrak{D})$. These concepts are evolved in all detail with motivation, illustrations, and applications in the forthcoming dimonograph (14).

We now exemplify directional duality with a theorem motivated by the study of pecking sequences in chickens. A formal theory for the systematic study of some of the properties of pecking sequences is developed by Rapoport (20), where further references to pecking studies may be found. We omit the proof of the following theorem, which will appear in (14).

Theorem. If every pair of points of $\mathfrak{D}$ are joined by a line in at least one direction, then there exists a point P such that the distance from P to any other point is less than 3.

To this theorem, there corresponds the:

Dual Theorem. If every pair of points of $\mathfrak{D}$ are joined by a line in at least one direction, then there exists a point of Q such that the distance from any other point to Q is less than 3.

Another example of directional duality, of interest in organization theory, involves the status of a person. In the paper (12), a formula is proposed for the status of a person P based not only on the total number of subordinates, but also on the distance from P to each of his subordinates. More precisely, the *status* $s(P)$ of a person P may be defined

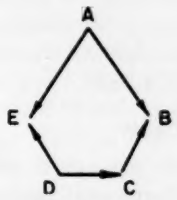

FIG. 5. Illustration of point basis and point contrabasis in a digraph.

by the following formula, in which $d(P, Q)$ denotes the distance from P to Q:

$$s(P) = \sum d(P, Q). \qquad [11]$$

where the summation ranges over all the persons Q in the organization. Then one can define the *contrastatus* $s^{\vee}(P)$ by the dual formula:

$$s^{\vee}(P) = \sum d(Q, P). \qquad [11']$$

As the formula indicates, the contrastatus of a person gives an indication of the amount of power or influence which all other persons have over him. It is our contention, as expressed in (12), that an accurate representation of the power position of a person in an organization must necessarily take not only status but also its directional dual, contrastatus, into consideration. In the paper (25), a similar theory using the terminology of "control" is developed by Tannenbaum. Here "having control over" (active) and "being controlled by" (passive) correspond approximately to our concepts of status and contrastatus respectively.

An application of directional duality to social psychological theory is provided by French (8) by means of his postulate to the effect that a liking relation on a group induces a converse influence relation. Expressed for pairs, whenever A likes B, B has (social) influence over A. As a consequence of this, each contra-concept in the social structure of a group is equivalent to a concept in its influence structure.

III. The third kind of structural duality has as its setting relations which are sometimes positive and sometimes negative. The appropriate kind of graph for such relations is introduced and studied in the papers (6, 11). In a signed graph, some of the lines are positive, and the remaining lines are negative. The *negation* $\mathcal{S}^-$ of the signed graph $\mathcal{S}$ is obtained from $\mathcal{S}$ by changing the sign of each line of $\mathcal{S}$. Clearly, the negation of the negation of $\mathcal{S}$ is $\mathcal{S}$ itself.

Antithetical duality

Each theorem on signed graphs is transformable into another theorem on signed graphs when one replaces every concept by its negation.

A *cycle* of a graph consists of a path AB, BC, $\cdots$, DE, together with the line EA joining the two endpoints of the path. A signed graph is *balanced* if every cycle is positive, i.e., has an even number of negative lines. The fundamental theorem on balanced signed graphs [proved in (11), and in disguised form in (17)] is the:

Structure Theorem for Balance. A signed graph is balanced if and only if its points can be separated into two mutually exclusive subsets such that each positive line joins two points of the same subset and each negative line joins points from different subsets.

The concept which is antithetical to balance is that of antibalance. A signed graph is *antibalanced* if every cycle has an even number of positive lines. Applying the law of antithetical duality, we immediately have without further proof the dual of the preceding structure theorem:

Structure Theorem for Antibalance. A signed graph is antibalanced if and only if its points can be separated into two mutually exclusive subsets such that each negative line joins two points of the same subset and each positive line joins points from different subsets.

One justification for the study of antibalance is that the relation under consideration may have negative connotations, e.g., the relation "dislikes," whereupon its opposite or antithetical relation (in this case "likes") would then be regarded as being represented by the negative lines of a graph. An interesting aspect of balance arises from the consideration of self-duality. We say that a signed graph is *duobalanced* provided it is both balanced and antibalanced. In this case, every cycle must have even length since it contains both an even number of negative lines and an even number of positive lines. Another example of two antithetical relations is that of "power" and "negative power," (as suggested in 8). One interpretation of duobalanced structures is that regardless of whether the original relation is intrinsically positive or negative, the structure is in a balanced state.

The diagram of Figure 6 presents a condensed form of a balanced signed graph in accordance with the Structure Theorem for

FIG. 6. A balanced signed graph.

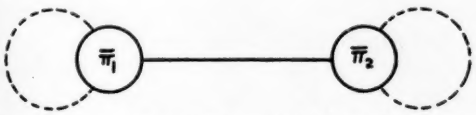

FIG. 7. An antibalanced signed graph.

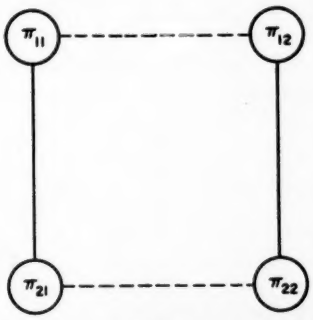

FIG. 8. An illustration of duobalance.

Balance. The points π_1 and π_2 represent the two subsets, the negative line joining them stands for any negative line of the graph, and the two positive lines joining π_1 with itself and π_2 with itself are condensations of all the positive lines. Similarly, Figure 7 is the prototype for all antibalanced signed graphs, following the Structure Theorem for Antibalance. Combining these two theorems, we are led directly to a corresponding theorem for duobalance, which is indicated in condensed form in Figure 8. If π' and π'' are disjoint point sets, a π'–π'' line means a line joining a point of π' with one of π''.

Structure Theorem for Duobalance. A signed graph is duobalanced if and only if its point set π can be partitioned into four mutually exclusive subsets π_{11}, π_{12}, π_{21}, and π_{22} such that

(*a*) there are no lines joining two points of the same subset, or π_{11}–π_{22} lines, or π_{12}–π_{21} lines,

(*b*) all positive lines are π_{11}–π_{21} lines or π_{12}–π_{22} lines, and

(*c*) all negative lines are π_{11}–π_{12} lines or π_{21}–π_{22} lines.

To prove this theorem, we use the definition of duobalance, which entitles us to apply both of the previous structure theorems. Let π_1 and π_2 be the two point sets induced by balance and $\bar{\pi}_1$ and $\bar{\pi}_2$ be the two point sets induced by antibalance as in Figures 6 and 7 respectively. Then we define a partition of π into four mutually exclusive point sets as follows. Let

$$\pi_{11} = \bar{\pi}_1 \cap \pi_1,$$

$$\pi_{12} = \bar{\pi}_1 \cap \pi_2,$$

$$\pi_{21} = \bar{\pi}_2 \cap \pi_1,$$

$$\pi_{22} = \bar{\pi}_2 \cap \pi_2.$$

(*a*) There are no π_{ij}–π_{ij} lines (i.e., lines joining two points of the same subset) since any such line would have to be both negative since it joins two points of $\bar{\pi}_i$ and positive since it joins two points of π_j. Similarly if there is a π_{11}–π_{22} line or a π_{12}–π_{21} line, it is positive since it is a $\bar{\pi}_1$–$\bar{\pi}_2$ line and it also is negative since it is a π_1–π_2 line.

(*b*) Since all positive lines are $\bar{\pi}_1$–$\bar{\pi}_2$ lines, and since there are no π_{11}–π_{22} or π_{12}–π_{21} lines, every positive line must be a π_{11}–π_{21} line or a π_{12}–π_{22} line.

(*c*) Similarly all negative lines are π_{11}–π_{12} lines or π_{21}–π_{22} lines.

It is interesting to observe that there is a psychoanalytic school of thought which holds that the difference between loving and hating is less than that between loving and indifference. One possible support for this viewpoint is that both loving and hating are intense feelings while indifference involves no feeling at all.

DERIVED DUALITY LAWS

There are two additional duality laws derivable from the three previous kinds of duality. These are "uncontra-duality" which combines existential and directional duality, and "anticontra-duality" which is obtained from antithetical and directional duality. Before stating these explicitly, we show that existential and antithetical duality are not combinable. In view of this fact, it is clear that the two above-mentioned combined duality laws are the only ones derivable from the three basic kinds of duality treated in the preceding section.

FIG. 9. A signed graph and two of its complements.

If existential and antithetical duality are combinable, then both the negation and the complement of a signed graph must be uniquely defined. The negation of a signed graph is completely determined since it is obtained from the given graph by changing the sign of every line. However, the complement of a signed graph is not uniquely determined since there are two possible choices for the sign of each line in it. This is illustrated in Figure 9, where both Figure 9b and Figure 9c (among others) are possible candidates for the complement of Figure 9a.

For "uncontra-duality," we recall the equation [10], $\mathfrak{D}'^{\smile} = \mathfrak{D}^{\smile\prime}$, which asserts that the unary operations of complement and converse of a digraph commute with each other. Both this and "anticontra-duality" are special cases of the:

Combined Duality Theorem. If S^* and $S^\#$ are different unary operations on S which commute with each other:

$$S^{*\#} = S^{\#*}, \qquad [12]$$

each of which induces a duality, then the product of these two operations, given by either member of equation [12] also induces a duality.

Proof. By hypothesis $S^{**} = S$, $S^{\#\#} = S$, and $S^{*\#} = S^{\#*}$. Therefore $(S^{*\#})^{*\#} = (S^{**})^{\#\#} = S^{\#\#} = S$. Further, if both the $*$-dual and the $\#$-dual of a statement concerning S are true, then so is the corresponding $*\#$-statement.

By this combined duality theorem, we know that "anticontra-duality" is a duality provided we know that negation and converse of a signed digraph commute with each other:

$$\mathfrak{D}^{-\smile} = \mathfrak{D}^{\smile -}. \qquad [13]$$

This follows immediately from the fact that both members of Equation 13 represent the signed digraph obtained from $\mathfrak{D}$ when both the direction and the sign of every line is changed.

Now that we have established that these two combinations of duality are themselves dualities, we state them formally.

IV. *Direxistential Duality Theorem. If one takes both the complement and the converse of each concept in a theorem about digraphs, the resulting statement is a theorem.*

We illustrate this kind of combined duality with the concept of a transmitter of a digraph. The *input* of a point P of a digraph is the number of lines to P; the *output* of P is the number of lines from P. A *transmitter* is a point of zero input; a *receiver* is a point of zero output. Thus a *contratransmitter* is a receiver. Hence P is an *uncontratransmitter* of $\mathfrak{D}$ whenever P is an unreceiver of $\mathfrak{D}$, that is, a receiver of $\mathfrak{D}'$. Such a point P is characterized by the property that the output of P is one less than the number of points of $\mathfrak{D}$.

V. *Directional-Antithetical Duality Theorem. If one takes both the converse and the negation of each concept in a theorem about signed digraphs, the resulting statement is a theorem.*

We can exemplify this last kind of duality for signed digraphs with the set of all points P such that there is a positive directed line from a given point A to P. If we denote this set by $\pi_+(A)$, then the dual set $\pi_-^{\smile}(A)$ is the set of all points Q such that there exists a negative line from Q to A.

MATRIX DUALITY

We have discussed five kinds of structural duality, namely: I. existential, II. directional, III. antithetical, IV. direxistential, and V. directional-antithetical. To each of these, there is a corresponding matrix duality. The matrix operations needed to describe these are as follows:

The matrix U is that n by n matrix in which every entry is a one.

The transpose, M^t, of the square matrix M is obtained from M by reflection about the main diagonal.

The negative, $-M$, of M is that matrix in which each entry is the negative of the corresponding entry of M.

For structural dualities I, II, and IV, we stipulate that the matrices have entries

which are either 0 or 1. For the two remaining dualities, the entries may range over -1, 0, or 1. The five corresponding matrix dualities may then be described by the following one-to-one mappings (indicated by the double arrows):

$$\text{I. } M \leftrightarrow U - \mathrm{M}$$

$$\text{II. } M \leftrightarrow M'$$

$$\text{III. } M \leftrightarrow -M$$

$$\text{IV. } M \leftrightarrow U - M'$$

$$\text{V. } M \leftrightarrow -M'$$

There is actually a correction factor which should be added to some of these if we exclude structures in which there is a line from a point to itself, but we omit this readily handled complication for simplicity.

DUOPROPERTIES

We have already encountered the concept of a duobalanced structure, namely one which is both balanced and antibalanced. Are there duoproperties for the other kinds of duality?

For existential duality, we consider the three concepts of connectedness, isomorphism, and liaison person. A *duoconnected* structure is one whose graph $\mathcal{G}$ is connected, and whose complementary graph $\mathcal{G}'$ is also connected. For example, in Figure 1 both graphs are connected but only the second one is duoconnected (see Figure 1 for the complementary graphs).

Two graphs are *isomorphic* if there is a one-to-one correspondence between their point sets which preserves adjacency. A group is *duostructured* if its graph $\mathcal{G}$ is isomorphic to its complement $\mathcal{G}'$. For example, the graph $\mathcal{G}_2$ of Figure 1 represents a duostructured group. Another example is given in Figure 10.

It can be readily be shown that there do not exist any duoliaison persons! This fact is contained in the:

Theorem. If A is an articulation point of $\mathcal{G}$, *then A is not an articulation point of* $\mathcal{G}'$.

We omit the proof, but remark that there do exist groups having both a liaison person and an unliaison person. The theorem does show that these two roles cannot be filled by the same person.

We mention briefly a trivial example of a duoproperty for directional duality. A point P is a duotransmitter (or a duoreceiver) if and only if P is both a transmitter and a receiver; hence duotransmitters coincide with isolated points of a digraph.

FIG. 10. A graph of a duostructured group.

OTHER DUALITIES

There may exist additional kinds of structural duality of interest in the study of behavioral science. We sketch some of these briefly using the language of graph theory; the translation to the concepts of interpersonal and other relations offers no difficulty:

(*a*) A *sling* is a directed line from a point to itself. If some of the points of a thus generalized digraph $\mathfrak{D}$ have slings and the others do not, then the digraph obtained from $\mathfrak{D}$ by interchanging the presence and absence of slings provides the setting for a new kind of duality.

(*b*) If $\mathfrak{S}$ is a signed digraph with slings (either positive or negative) permitted, then the operation of negating each sling is a duality.

(*c*) Related to this last idea is that of "signed points." If each point has a positive or negative "valence," then changing the sign of the valence of each point constitutes a dual process.

(*d*) If there are two different relations on the same set of points, i.e., if we have a *graph of type 2*, we can interchange these two relations to obtain a duality rule.

(*e*) These dualities are sometimes combinable to obtain new ones.

We note that there are other kinds of matrix duality, for example

(*a*) the inverse of a nonsingular matrix,

(*b*) the complex conjugate of a matrix whose entries are complex numbers, or

(*c*) the matrix obtained from a given matrix M all of whose entries are nonzero, on

taking the reciprocal of each entry, etc. But none of these appears behaviorally applicable.

In matrix theory, if M is a matrix whose elements are complex numbers, then M is a symmetric matrix if $M = M^t$, M is "skew-symmetric" if $M = -M^t$, M is "Hermitian" if it is equal to the transpose of its complex conjugate. These are all examples of "self-duality." We have noted previously that $M = M^t$ and $M = -M^t$ correspond respectively to directional self-duality and, directional-antithetical self-duality.

An example of structural self-duality has been given by duostructured groups; these are self-dual existentially. An ordinary graph (i.e., one with no directed lines) is self-dual directionally. This leads us to the expected generalization that a structure is *self-dual* with regard to a given duality if it is its own

FIG. 11. A signed graph that is self-dual antithetically.

dual. For example, any signed graph consisting of exactly one alternating cycle, as in Figure 11, is self-dual antithetically.

We mention as a question for possible future investigation the possibility of the existence of interesting kinds of structural triality, etc. Two kinds of structural triality which suggest themselves are:

(*a*) Given three binary relations (R_1, R_2, and R_3 on the same set of points, the process of transforming the three different types of lines cyclically: $R_1 \rightarrow R_2 \rightarrow R_3 \rightarrow R_1$ provides an example of structural triality.

(*b*) To obtain an analogy with existential duality, one can consider a kind of graph [of strength two; see (13)] in which any pair of points may be joined by either no lines, one line, or two lines. Then the procedure of adding one additional line joining each pair of points, with the stipulation that three lines are regarded as no lines, is a structural triality which can be properly called "existential triality."

Whitney (30) defined the dual of a planar map, e.g., the map of continental Europe, by essentially the following construction. The dual graph of the map of Europe has a point standing for each country, and two points are adjacent in this graph whenever the corresponding countries have a common boundary in the map. This kind of duality, which is of considerable importance in electrical network theory, was rediscovered by Bavelas (2) in his formalization of parts of Lewin's work (19).

We conclude by mentioning two more kinds of physical duality, only the first of which has been found empirically thus far. *Magnetic duality* is illustrated by the discovery of the "antineutron" (23), which has the same mass as the neutron, but opposite magnetic orientation. *Gravitational duality* has been hypothesized by some astrophysicists. This theory proposed the existence of antimatter which attracts other antimatter gravitationally, but repels ordinary matter.

We mention *en passant* part of a delightful letter from a well-known physicist to a well-known magazine (26). We note that the prefix "anti" in this quote refers always to physical charge duality:

> In a recent issue of *The New Yorker*, I found the following poem, describing the meeting of Dr. Edward Anti-Teller with an imagined person differing from Anti-Teller only in the sign of the charges carried by the particles in his body.
>
> Perils of Modern Living
>
> Well up beyond the tropostrata
> There is a region stark and stellar
> Where, on a streak of anti-matter,
> Lived Dr. Edward Anti-Teller.
>
> Remote from Fusion's origin,
> He lived unguessed and unawares
> With all his anti-kith and kin,
> And kept macassars on his chairs.
>
> One morning, idling by the sea,
> He spied a tin of monstrous girth
> That bore three letters: A. E. C.
> Out stepped a visitor from Earth.
>
> Then, shouting gladly o'er the sands,
> Met two who in their alien ways
> Were like as lentils. Their right hands
> Clasped, and the rest was gamma rays.
>
> —H. P. F.

I was pleased that *The New Yorker* mentioned me. Come to think of it, only Anti-Teller was mentioned by name in the poem, but I am con-

fident that somewhere in an anti-galaxy *The Anti-New Yorker* devoted some pleasant lines to

Yours sincerely,
EDWARD TELLER

Finally, we mention a very recent development. A critical experiment appears to imply the necessity of abandoning the physical directional and charge duality laws (21)!

The two concepts definitely believed destroyed are the principle of parity and an allied principle called charge conjugation. This indicates that a particle and its corresponding antiparticle will decay in an identical manner (27).

Regardless of the potentially great effect of these results on physical theory, the derivation of measurement techniques for the behavioral sciences which are sufficiently reliable to perform analogous critical experiments testing the validity of structural dualities seems to be in the distant future.

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COMMUNICATION SYSTEMS AND SOCIAL SYSTEMS. A STATISTICAL EXPLORATION IN HISTORY AND POLICY[1]

by Daniel Lerner

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Heretofore communication systems have evolved and adapted themselves to the development and growth of the social system. In this article the relationships between four factors important in such development and growth are discussed. It is pointed out that some societies, over-zealous to attain modernization, may attempt radical departure from these historical relationships with resultant imbalances and possible breakdowns in the social system.

PEOPLE who live together in a common polity develop patterned ways of distributing information, as of distributing other commodities. These patterns of information flow interact at many points with the patterns of power, wealth, status, and other values to form a *system*, i.e., institutional variation in one is accompanied by regular and determinate variations in the others. This paper aims to determine the degree of systemic relationship between communication and other institutions in most of the societies around the world.

We have identified two main types of public communication systems—media and oral. These are differentiated according to the paradigmatic question of communication research: who says what, how, to whom? On these four variables the differences are as follows:

	Media Systems	*Oral Systems*
Channel	Media (Broadcast)	Oral (Point-to-Point)
Audience	Mass (Heterogeneous)	Primary (Homogeneous)
Source	Professional (Skill)	Hierarchical (Status)
Content	[Descriptive][2]	[Prescriptive][2]

Media systems have been described in detail by communication specialists. The main flow of public information is activated by a professional corps of communicators, skilled in producing descriptive messages ("news") for transmission through impersonal "media" (print, film, radio) to relatively undifferentiated mass audiences.

Oral systems we know mainly from the reports of anthropologists. Since preliterate networks are considerably more diverse than media systems (which have an "homogenizing" effect on behavioral styles) their public institutions exhibit much variation. In some oral systems, for example, power is not rigidly hierarchized. In the modal type, however, messages usually emanate from sources authorized to speak by their place in the social hierarchy, i.e., by status rather than skill criteria. These messages typically appear to be prescriptive rather than descriptive, i.e., announcing the regulations that are to govern audience behavior toward imminent events of community-wide interest, such as tax collections and military drafts. They are transmitted through oral channels to highly differentiated audiences, i.e., the "natural" primary groups of kinship, worship, work, or play. Each of these

[1] I wish to thank R. S. Eckhaus, E. E. Hagen, B. F. Hoselitz, H. D. Lasswell for suggestions which have been incorporated in this paper. Case studies of the theoretical structure here outlined are reported in the author's forthcoming book entitled *Modernizing the Middle East: Studies in communication and social change*.

[2] These are bracketed because the only systematic evidence of content variation between the two types available to me is confined to a study made in the Middle East several years ago. I suspect, but cannot demonstrate, that there is regularity in this variation around the world.

groups completes the diffusion pattern of an oral network by acting as a relay channel of mouth-to-ear communication within and between groups.[3]

If we accept this terse formulation as satisfactorily differentiating the characteristics of two general models of communication systems, we come next to the problem of describing their occurrence with sufficient accuracy to discriminate consistently between media and oral systems in the observable world. Here we run into trouble, for there are few societies that give a perfect fit to either of these idealized sets of paired comparisons. For example, in Britain, where public communication approximates most closely the model of a media system, people still talk to each other about public issues. Conversely, in Saudi Arabia, which corresponds to the oral system, there is a radio station. In most societies, as we move from the ideal types into empirical data, various elements in the patterns begin to shift. Most societies in the world appear to be in some stage of transition from one pattern to the other.

We notice, however, two general features that appear to be common to all societies. First, the *direction* of change is always from oral to media system (no known case exhibits change in the reverse direction).[4] Second, the *degree* of change in communication behavior appears to correlate significantly with other behavioral changes in the social system. These observations indicate that we are dealing with a secular trend in communication systems, a long-term process of historical change that is unilateral in direction. Moreover, this trend appears to be systemic, since it occurs interdependently with a variety of non-communication factors. From this we derive the proposition that a communication system is both index and agent of change in a total social system. Leaving aside the genetic question of causality (on the view that once the process is started, chicken and egg in fact "cause" each other), the hypothesis may be formulated in a suitable manner for testing as a correlation matrix, viz:

	Type I	*Type II*
Communication	media	oral
Socioeconomic	urban	rural
Political	representative	nonrepresentative
Cultural	literate	illiterate

Associated with each communication system is a "profile" of economic, political, and cultural attributes. To sharpen the differences, they are stated above in dichotomous fashion. The dogmatic character of such a formulation need trouble no one, however, for empirically we treat them as continuous variables, on which differences are calibrated. Just as there is no perfect media system, so there is no perfectly urban or perfectly representative or perfectly literate society. Our model is probabilistic, our measures are distributive, and our test of fit is correlational.

The procedure was to determine the actual degree of correlation among these indices, for all societies of the world which supplied data. These indices were defined in such fashion as to permit maximum use of the statistical data reported by UNESCO and other U.N. agencies. As the number of countries reporting varies from one index to another, our correlations apply to groups of nations ranging from 54 to 73 in number.[5]

[3] Comment by H. D. Lasswell: "One interesting variant is the role of drumming and similar media channels in predominantly oral systems. Such modes of communication are simply telegraphic devices more like conventionalized signposts than expressive or prescriptive statement-making. The sign-symbol ratio is high, by which I mean the physical events that serve as channel are rather extensive. A visual system seems essential to cut down the sign-symbol ratio and approach the 'pure' sign. Such visual systems may be 'manufactured' when they appear and copyists arise. The demand must be great—as at a dominant capital or at another urban locus."

[4] This discussion excludes the new totalitarian systems, which have revived the ancient importance of the Agitator, using oral modes of communication. This special case appears to fill a "communication gap" created by the excessive development of capacity to produce messages through the media (especially print) over the capacity to consume mediated messages (e.g., by reason of illiteracy or lack of equipment).

[5] The statistics were used as reported by UNESCO (7). This was checked wherever possible against other UNESCO sources (for typographical errors) and against the U.N. *Statistical Yearbook* (10) and *Demographic Yearbook* (11) (for errors of information and computation). Wherever signifi-

Each index is considered a reliable guide to the state of public participation in its "sector" as a whole. Thus the literacy index, by specifying the proportion of population which can read in one language, is considered to give a fair picture of national participation in the whole cultural sector. Also, the proportion of population actually voting in national elections indexes participation in the whole political sector. Similarly, urbanization, computed as the proportion of population living in cities over 50,000, is taken as an index of participation in the whole economic sector. While urbanization is usually taken more narrowly, as a measure only of occupational distribution, a broader interpretation can be based on previous studies showing high intercorrelations between occupational distribution, per capita income, and literacy.[6]

These indices express degree of participation within four sectors which, in this discussion, can be taken to represent the whole social system. What they differentiate is the participant style of modern democratic societies from the nonparticipant ways of traditional hierarchic societies. By "participant style" we mean here the *frequency*, not the quality, of participation by individuals. The point is simply that *more* individuals receive and use the opportunity to participate, regardless of the "value" of their participation. Accordingly, the items selected to form the communication index also focus on frequency of participation by the general population. These items are: (*a*) circulation of daily newspapers; (*b*) number of radio receivers; (*c*) seating capacity of cinemas. Each of these items was first correlated separately with the other indices. After their separate coefficients had been determined, the three items were handled jointly as a single index. (In all cases they were expressed, for comparability between items and countries, as proportion per 1,000 population.)

TESTING THE COMMUNICATION ITEMS

Each of the three communication items was correlated with the cultural index of literacy.[7] The coefficients, for a group of 73 self-governing countries, were as follows:

Item	*Correlation with Literacy*
Daily newspaper circulation	.75
Number of radio receivers	.74
Cinema seating capacity	.61

It is obvious that newspaper circulation should correlate better with literacy than does movie attendance, the enjoyment of which does not require literacy. The high correlation of radio receivers leads, for explanation, in another direction. Whereas building cinemas (in which *imported* feature films are shown) requires no advanced technology, the mass production of radio receivers does require a fair rate of industrialization on a high technological level.

The differential rate of industrialization is subsumed, up to a certain determinate point, under our index of urbanization. Having established that a high correlation exists between literacy and media, we now seek to establish that urbanization is interdependent with both. Rising production and distribution of the media usually occur only where and when there is the minimal urbanization required for modern industrial processes. By the same token, urbanization requires rising literacy for industrial participation. At a certain point, when urbanization has done its work, literacy becomes

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cant differences appeared which could not be reconciled, the case was excluded from our analysis. Otherwise all "self-governing territories" are included. The writer is unable to offer any definitive evaluation of these U.N. data, which are assembled from reports prepared separately by each nation. There are national differences in definition of indices and accuracy of reporting. Whereas magnitudes of "error" cannot be checked systematically, the direction of error, in those cases I have checked, always tend toward overstating one's progress in modernization—i.e. underdeveloped countries are likely to report larger rather than smaller estimates of urbanization, literacy, voting, etc.

[6] Literacy around the world correlated at .84 with per capita income, at .87 with industrialization measured by proportion of gainfully employed males in non-agricultural occupations. See Refs. 2, 9.

[7] Literacy is reported by UNESCO in five categories, each covering a range of 20 percentage points. All other items were handled as ungrouped data. The standard deviation for the literacy distribution is high (31.4).

the independent variable in the process of growth and a new phase of modernization begins. But the growth of literacy itself, in this phase, soon becomes closely associated with the growth of media. The media teach literacy, and growing literacy develops the market which consumes the media product. The high coefficients correlating literacy with each of the media suggest that these may be considered as reciprocal causes and effects in a communication market whose locus can only be, at least in its historical inception, urban.[8]

The role of cities becomes clearer if we consider the further suggestion that sheer density of population, without countervailing urbanization, tends to operate as an anti-literacy force in most societies. This appears to be so despite the fact that education is cheaper when pupils live close together and hence, other things being equal, density should be associated with greater literacy. But, in the absence of significant urbanization, other things are *not* equal—i.e., the production, distribution, and consumption of wealth are much lower. This has a direct depressing effect on all public services, notably free public education. In dense nonurban societies, where national income is relatively small, few schools are maintained by public funds; also, since per capita income is lower and less widely distributed, fewer individuals can afford to attend school (5). Hence, the more people there are in a given area, the smaller is the proportion being educated and the harder it is to get a rising proportion of literates among them—until they begin to be redeployed into cities. In sparsely settled lands the influence of urbanization is less marked and literacy rates will probably respond directly to rises in per capita national income. But in populous societies urbanization is the intervening variable and is crucial for the "take-off" toward increasing literacy. It appears that only when dense populations show a significant rate of urbanization do literacy rates begin to rise. The rise of literacy levels off, however, after a certain degree of urbanization is present in the society. This means that the continued growth of literacy—say, after the society has become half-literate—depends upon some factor other than the continued growth of cities.

The counter-literacy force of sheer population density is evident in the populous Asian societies, such as India and Indonesia, where significant rates of urbanization have not yet occurred. The suggested interplay of density and urbanism as factors conditioning literacy may be represented as follows:

		Urbanism	
		High	Low
Density	High	High (Literacy)	Low (Literacy)
	Low	High (Literacy)	High/Low (Literacy)

To facilitate testing of these relationships between density, urbanization, and literacy, we formulated three distinct hypotheses: (*a*) that literacy and population density, in areas of low urbanization, vary inversely and exhibit a negative correlation; (*b*) that the rate of literacy increases positively as the degree of urbanization increases (whether density is low or high); (*c*) that when urbanization exceeds a determinate figure then literacy will be high, regardless of population density, but will no longer be raised simply by rising urbanization. (No hypothesis is offered under the fourth set of conditions, where *both* urbanization and density are *low*, the impact of rising urbanization upon literacy being indeterminate in this case.)

[8] Comment by B. F. Hoselitz: "I tend to believe that literacy is the independent variable, at least in some cases, since the general experience in South Asia seems to be that the proportion of literates among migrants to cities is greater than among the rural population whence they came. In other words, people with a higher degree of empathy engage in migration to towns. On the other hand, the urban development, partly because of the availability of better schooling facilities, tends to produce a higher degree of literacy. One would, therefore, have to make a distinction between literacy rates of those who were born, or at least lived, in the urban environment since they were of school age, and those who migrated to the city as grownups."

A more complex formulation would take these three hypotheses together and would seek to determine the triadic conditions under which monotone relations actually obtain between urbanization, density, and literacy. Here we take the simpler course of testing the pairwise relationships by correlation. Our main interest here being to establish the crucial role of urbanization in the early phase of modernizing a social system, we suggest its differential functioning under conditions of low and high density only as a lead for future investigation. In computing these correlations, population density was defined as the number of persons per square kilometer of territory, and urbanization was defined as the proportion of total population living in cities over 50,000. Correlation of literacy with population density gave us a negative coefficient of −.60. Inspection of the two sets of figures showed that this inverse relationship was due to the massive nonurban societies—China, India, Indonesia, Egypt, etc. (cf. 8). This may be regarded as confirmation of the first hypothesis that sheer density of population, in areas of low urbanization, is a counter-literacy factor—as density rates increase, literacy rates tend to decrease.

Confirmation of the second hypothesis, that literacy *increases* as urbanization increases, was also clear from the coefficient of correlation between these variables: +.64. (It should be noted that inaccuracies in the raw statistical data tend to bias the results against this hypothesis. Density is computed by formal territorial jurisdiction rather than effective area of habitation; since the densest countries tend to have the largest "waste" areas this minimizes their actual density. Also the cutting point of 50,000 excludes many cases of genuine urbanization in the less dense and populous countries, where cities of smaller size represent a significant degree of urbanization. Hence, making the raw data more accurate would tend to raise all coefficients in the direction hypothesized.)

Our third hypothesis, that after a certain point in urbanization has been passed literacy is high regardless of other demographic variables, was made more plausible (though not completely confirmed) when we ranked all 73 countries with respect to literacy and urbanization.

Number of Countries	*Literacy*	*Urbanization (Mean)*
22	Over 80%	28.0%
4	61–80	29.2
12	41–60	25.0
13	21–40	17.0
22	Under 20	7.4

Clearly, urbanization is an important factor up to the point at which one-fourth of the population lives in cities over 50,000. The direct and monotonic relationship between literacy and urbanization (the surplus of 1.2% in the second row, which contains only four countries is insignificant) is clearest from the time urbanization reaches 10% until it passes 25%. Beyond this point urbanization levels off, while literacy continues to rise "independently" (in countries of extremely high *and* extremely low population density).

If we take 10% and 25% as approximate cutting points in the scale of urbanization, we are able to classify societies into three categories which also discriminate quite consistently the degree of literacy and media participation in each society. Let us designate these three categories as Modern, Transitional, Traditional, to mean the following:

	Literacy	*Urbanization*	*Communication System*
Modern	Over 61%	Over 25%	Media
Transitional	21–60	10–25	Media-Oral
Traditional	Under 20	Under 10	Oral

These cutting points are somewhat arbitrary, of course, in the sense that their outcome is partly determined by the statistical input. (Had urbanization been indexed by cities over 20,000 rather than 50,000 population, for example, the upper cutting-point on this continuum might well be located at 20% rather than 25%.)

The results do enable us, however, to specify two main phases in the process of secular change toward a participant social system. The first phase, speaking summarily, is urbanization. It is the transfer of population from scattered hinterlands to urban

centers that provides minimum conditions needed for "take-off" toward widespread participation. Only cities have developed the industrial complex of machines and skills which produces, among other things, newspapers and radio networks and motion pictures. In this first phase, accordingly, increases in urbanization tend in every society to be accompanied by increases in the production and availability of communication media. Once the basic industrial plant is in operation, however, the development of a participant society passes into a subsequent phase. Increasing urbanization, once having provided the initial conditions of production, no longer automatically assures equivalent increases in consumption. The need now shifts to increasing the conditions which govern consumption.

Of this next phase, literacy is both the index and agent, since literacy provides the basic skill required for operation of a media system. Only the literate produce the media contents which, as our literacy-media correlations showed, mainly the literate consume. Hence, in societies which are about 25% urbanized, the highest correlation of media consumption is with literacy. We shall soon explain more clearly why literacy is the pivotal agent in the transition to a fully participant modern society. Here we wish to stress, in summary fashion, that by the time this modern phase gets well under way, a different social system is in operation than that which governed behavior in a society that was under 10% urbanized and under 40–60% (roughly, less than half) literate. With higher literacy and media participation comes also increasing availability and use of facilities for participation in all sectors of the social system. An index of this is political participation, which reaches its most developed expression in governance by representation. We refer here to representation that reflects popular choice between real alternatives made through an electoral mechanism that gives one equal vote to every person defined as adult ("universal suffrage").[9]

[9] Accordingly, plebiscite-type elections of the sort staged in totalitarian systems are not included in the computations reported below.

THE SYSTEMIC TESTS

To test the hypothesized relationships set forth above, a four-index correlation matrix was used. To the three indices of urbanization, literacy, and communication (media participation) as already defined, a fourth index of political participation was added. This consisted of the proportion of *actual* voters to total population, from the most recent election back through a maximum of five elections. The figure for each country is the average of the voting figures in all elections recorded.

In addition to simple correlations between each pair of indices, multiple correlations were computed using each index in turn as the "dependent" variable for the whole matrix. For the latter operation, only those countries could be included which reported comparable data on all four variables. This reduced the total number of countries, in the multiple correlations, to 54. As this eliminated the countries with the least adequate statistical records, which tend also to be the "least developed" countries, this skewed all distributions involving urbanization—since urbanization is an important factor only during the phase when a society is developing its communication media, but not before nor particularly after this phase. Thus, with $N = 73$ the coefficient of correlation between urbanization and media participation was .47; with $N = 54$ the coefficient jumped to .58. This is a significant increase, as we would expect, because the developed nations have achieved "optimum" relations between urbanization and literacy, while the developing nations exhibit variant growth patterns (deviations from the regression lines)—some more urban than literate, others the reverse.

The high pairs of simple correlations for $N = 54$, again as hypothesized, were those involving literacy. With voting, literacy correlated at .80. With media, literacy correlated at .82.[10] Between these two variables, at all levels, there is a systemic interaction affecting the distribution of information in a manner akin to the production-consumption

[10] A noteworthy feature of these literacy correlations is their stability regardless of sample size: e.g., with $N = 73$, literacy and media correlated at .84.

reciprocal of any commodity in an expanding distribution system.

The essential demonstration of our hypothesis is in the results obtained by multiple correlation. This procedure enabled us to rotate each of the four factors in the matrix as the "dependent variable," thereby obtaining the simultaneous degree of relationship of each to all the others. The coefficients of multiple correlation, reported in turn for each dependent variable, were as follows:

Dependent Variable	*Multiple-Correlation Coefficient*	*Standard Error*
Urbanization	.61	.09
Media Participation	.84	.04
Literacy	.91	.03
Political Participation	.82	.05

Both the size of these coefficients and their ascending order are relevant. Their consistent significance demonstrates that the relationship between these four series of independent events is *systemic*—that rates of growth in these four sectors of the participant society have in fact "gone together" in most extant societies. Their ascending order supports the historical phasing that was sketched in the preceding pages.

Urbanization constitutes the initial phase of modernization. Our calculus indicates that at least 10% of the population must be urbanized before the "take-off" toward self-sustaining growth occurs in the other sectors. Thereafter, throughout the period we call the first phase, they tend to grow together. But after a certain degree of urbanization exists, and the society has taken off, then the further growth of cities no longer affects growth of other factors in the same degree. In our computation (based on cities over 50,000) this upper limit was 25%. Urbanization ceases to play a determinant role after this critical level has been reached. As our sample includes many countries which passed this 25% limit long ago, the urbanization correlations naturally yield the lowest coefficient.

Media participation, which indexes the adaptation of advanced technology to communication needs in the second phase of a modernizing society, obtains a considerably higher coefficient. The production and consumption of media products remain functional for growth long after urbanization has passed its dynamic limits. This was the phase in which the West developed, among other things, the "penny press" (cf. 4). This is the phase in which, as its characteristic functions mature, production of radio sets becomes a useful index of growth in the total industrial production (1).

The diffusion of literacy, which increases rapidly in the post-urbanization phase, produces an extremely high correlation coefficient. Literacy continues to be functional for growth at every step in the modernization process. For, with literacy, people acquire more than the simple skill of reading. They gain access, in the very act of achieving distance and control over a formal language, to the world of vicarious experience and the complicated mechanism of empathy which is needed to cope with it. (Throughout the Middle East, illiterate respondents often said of their literate compatriots words to the effect that "They live in another world.") This is why media consumption, in every country we have studied, exhibits a centripetal tendency. Those who read newspapers also tend to be the heaviest consumers of movies, broadcasts, and all other media products.

Thus literacy becomes the sociological pivot in the activation of psychic mobility, which is a modal personality attribute of participant society. Psychic mobility (i.e., a high capacity for empathy) is required in modern society, where much individual "participation" occurs in a universe of vicarious experience. The empathic capacity to relate oneself to a large variety of secondary symbols and public issues is a skill taught by the mass media. This is the psychocultural tie which binds together the diversified activities of modern man's daily round into a homogeneous "participant lifestyle." It explains why the literate urbanite has tended to be not only the main media consumer but also the cash customer and the voter. It explains why, historically, our variables have grown so closely together (3).

We come, then, to the diminished coef-

ficient of .82 obtained by political participation, which requires explanation. Political democracy comes late, historically, and typically appears as a crowning institution of the participant society. That the voting coefficient is lower than the literary coefficient indicates either that the historic link of voting has become everywhere somewhat less intimate than we have supposed, or that some recent "stochastic factor" is at work in some parts of the world. Such a factor appears to be the new global fashion to install some voting mechanism as a symbol of modern desires rather than as a functional agency of modern governance. Democracy has become a world fad, spread across national lines by diffusion, rather than an institutional fulfilment of needs developed internally by an increasingly participant society.

But policy cannot emulate Humpty Dumpty and make events mean just exactly what it wishes. The deliberate policy decision by many underdeveloped countries to induce certain traits of modernity willy-nilly, rather than to await their evolution in the historical sequence, produces "ahistorical" results. Some of these countries show inordinately high voting rates. Indeed, whereas high correlation of voting with the other variables occurs among the modern countries in our sample, the sharp deviations are largely accounted for by the figures reported from India and Southeast Asia, Turkey and the Middle East, South America. These are the great "underdeveloped areas," which have in common the poverty of their current resources relative to the soaring heights of their current aspirations. They are under-urbanized, under-technologized, under-literate, relative to their urge rapidly to move into the common practices of modern participant society. Thus, India in 1952 and again in 1957 offered universal suffrage in national elections to a population which is predominantly remote from the urban centers, outside the reach of mass media, and illiterate. So Egypt, in 1956, ordered its impoverished rural masses to "vote" in a single-option plebiscite, which gave Abdul Nasser the 97% endorsement common in such performances. Such "ahistoricity" raises questions.

HISTORY AND POLICY

If we take the present findings as reasonable evidence that historically, say over the past two centuries, there has been a systemic relationship between the rates of urbanization, literacy, voting, and media participation, what inferences seem appropriate for those interested in development programs for the retarded areas of the world? First, these findings cannot simply be disregarded; in the absence of *other factors* it is likely that radical departures from these historic relationships will lead to serious imbalances and possible breakdowns in the social systems which attempt these departures. India's rank order is very much higher on the indices of political and media participation (the latter reflecting its extraordinarily high film output) than could be "justified" by its urbanization and literacy rates. *If* "other factors" were equal, one might expect a severe relapse (or even collapse) of India's electoral system and film industry on the basis of these deviations from the historical regularities.

However, and this is the second inference to be drawn, there is no necessity for other factors to be equal. Our aim is not prophecy, but the formulation of an essential question: Since the stability of modern societies has been associated over past centuries with the whole "system" of participant behavior gradually evolved, how can these new societies-in-a-hurry hope to achieve stability while acquiring mobility? The question is not rhetorical; the writer does not believe that he knows "the" answer. History is a matter of secular trends, not eternal laws, and the persistence of a certain pattern of social change in the past does not mean that things must always be so. On the contrary, the very act of describing past trends, and clarifying the conditions of their occurrence, may provide the enlightenment needed to redirect the course of history by the pressure of wise policy. The historical "other factors" can be varied, within limits, to suit new policy goals. One new sort of "other factor" is the set of innovations used to orient the

huge electorate in the Indian election—i.e., the use of symbolic devices and other techniques not needed in cases where literacy and the franchise have gone together.

The widespread *planning* of economic development for India is another sort of new "other factor," which was not present in the societies whose historical evolution has been studied here. This could have the effect of raising per capita income rapidly among illiterate villagers, thereby enabling them to become participants in the newer media (radio, film, television) without moving to a city or going to school. This would produce a participant population radically different, on key attributes, from the historical population of participants. New institutions of social consensus and social control would be brought into being by such a rearrangement of the historic factors. The task of policy planners is to study the new deviations and foresee appropriate institutions—those which will enable them to change the historic patterns successfully.

In planning the future, it is likely that they will have to give special attention to the accelerated shift of many underdeveloped societies from oral to media systems of public communication. It is a distinctive innovation of twentieth century social change that, in much of the underdeveloped world, people are acquiring psychic mobility before they have acquired social mobility. When the modern West took shape, in the eighteenth century, the reverse condition obtained. Thomas Jefferson expressed a basic operating principle of democratic governance when he asserted that the franchise should be exercised by those with a stake in public policy (i.e., property) and with a capacity for forming opinions about policy decisions (i.e., education). Out of this conviction grew such democratic institutions for qualifying voters as the literacy test and the poll tax.[11]

Nowadays, the theory of public participation is changing—largely under the impact of the mass media. Atatürk was perhaps the century's shining model of how to stimulate take-off toward modernization in a traditional society by means of a "communication revolution." Many are following this path. How well they will succeed in working out new relationships for the participant society of the future remains to be seen. That they mean to try is clear from their conviction that the old relationships no longer apply. A clear declaration of independence from the historic regularities that governed relations between communication systems and social systems over the past two centuries was expressed, on January 7, 1957, by President Gamal Abdul Nasser:

> It is true that most of our people are still illiterate. But politically that counts for far less than it did 20 years ago.
>
> Literacy and intelligence are not the same thing. Radio has changed everything. Once the villagers had no knowledge of what was happening in the capital. Government was run by small coteries of people who did not need to take account of the reactions of the people, who never saw a newspaper or could not read it if they did.
>
> Today people in the most remote villages hear of what is happening everywhere and form their opinions. Leaders cannot govern as they once did. We live in a new world. (6).

[11] The next phase of this inquiry will be to determine the historic regularities and current deviations, in the relationships between our matrix and a direct economic measure such as per capita income.

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Science aims at constructing a world which shall be symbolic of the world of commonplace experience. It is not at all necessary that every individual symbol that is used should represent something in common experience or even something explicable in terms of common experience. The man in the street is always making this demand for concrete explanation of the things referred to in science; but of necessity he must be disappointed. It is like our experience in learning to read. That which is written in a book is symbolic of a story in real life. The whole intention of the book is that ultimately a reader will identify some symbol, say BREAD, with one of the conceptions of familiar life. But it is mischievous to attempt such identifications prematurely, before the letters are strung into words and the words into sentences. The symbol *A* is not the counterpart of anything in familiar life. To the child the letter *A* would seem horribly abstract; so we give him a familiar conception along with it. "*A* was an Archer who shot at a frog." This tides over his immediate difficulty; but he cannot make serious progress with word-building so long as Archers, Butchers, Captains, dance round the letters. The letters are abstract, and sooner or later he has to realise it. In physics we have outgrown archer and apple-pie definitions of the fundamental symbols. To a request to explain what an electron really is supposed to be we can only answer, "It is part of the *A B C* of physics."

The external world of physics has thus become a world of shadows. In removing our illusions we have removed the substance, for indeed we have seen that substance is one of the greatest of our illusions. . . . In the world of physics we watch a shadowgraph performance of the drama of familiar life. The shadow of my elbow rests on the shadow table as the shadow ink flows over the shadow paper. It is all symbolic, and as a symbol the physicist leaves it. Then comes the alchemist Mind who transmutes the symbols. The sparsely spread nuclei of electric force become a tangible solid; their restless agitation becomes the warmth of summer; the octave of aethereal vibrations becomes a gorgeous rainbow. Nor does the alchemy stop here. In the transmuted world new significances arise which are scarcely to be traced in the world of symbols; so that it becomes a world of beauty and purpose—and, alas, suffering and evil.

—A. S. EDDINGTON in *The Nature of the Physical World*

DISHARMONY IN FEDERAL GOVERNMENT

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"Among the numerous advantages promised by a well-constructed union, none deserves to be more accurately developed than its tendency to break and control the violence of faction. The friend of popular governments never finds himself so much alarmed for their character and fate as when he contemplates their propensity for this dangerous vice. He will not fail, therefore, to set a due value on any plan which, without violating the principles to which he is attached, provides a proper cure for it."—*The Federalist.*

MODERN federalism, which can be regarded as an alternative to imperialism in the expansion of governments, consists essentially of a twofold constitutional guarantee. One guarantee is of the independence and viability of the central government such that its officials are enabled to decide their problems without consulting the officials of the constituent governments. In the United States, as in most modern centralized federalisms, this means that national officials are chosen without the formal aid or intervention of state officials and that national officials govern directly the citizens who choose them (6). It is this independence of the central government from the constituent ones that differentiates modern centralized federalism from peripheralized leagues like the medieval confederations of Switzerland and Suabia, or the Dutch Republic of the seventeenth century, or even the American government under the Articles of Confederation, in all of which national officials were by one constitutional device or another forced to refer decisions on some national questions to the organs of constituent governments (8). (The distinction between centralized and peripheralized federalism is, of course, not so sharp as the naming of the categories may suggest. Even the Dutch Republic had some centralized features [e.g., the Council of State and the Boards of Admiralty] and the United States did not lose one of its severely peripheralizing customs until the notion of instructions to senators from state legislature ceased to have practical effect in the 1840's and 1850's.) (6)

The other guarantee is of the perpetual existence of the constituent governments such that they are enabled to maintain themselves (in the selection of officials, etc.) independently of the national government. It is this guarantee that distinguishes all federalisms, whether centralized or peripheralized, from unitary states like Great Britain, in which local government is—in legal theory, at least—wholly under the control of the central government.

In political practice, the net effect of the operation of these two guarantees is the existence of two sets of governments in the same place directing the same people. Not surprisingly, conflicts between the two sets have characterized all modern federalisms. There is no necessary reason that such intergovernmental conflict should occur. Indeed, it is theoretically conceivable that constituent and central governments independently pursue harmonious and logically coordinated policies. In actual fact, however, all modern federalisms have at one time or another experienced severe intergovernmental conflicts, such as the American Civil War or

[1] The senior author wishes to thank the Rockefeller Foundation for a grant that permitted him sufficient freedom from academic duties to work on this essay.

the attempt of Western Australia to withdraw from the Commonwealth. And less divisive bickering between the center and the periphery is an almost universal feature of the operation of federalisms. Besides conscious bickering, federalisms constantly suffer from a lack of integration between the policies of the states and the nation. Often this disharmony approaches the extreme instance of fiscal policy in the United States in 1934, when states curtailed expenditures as an economy measure while the nation increased expenditures as a means of combating depression (3). Frequently, as perhaps in this instance, such contradictory policies are adopted without intent to impede or embarrass; they merely occur by reason of differences in ideology and interest among the officials of the two sets of governments. One cannot escape the impression, therefore, that the institutional structure of most contemporary federalisms is highly conducive to intergovernmental conflicts and to a failure to integrate policies. Just how and why the institutions of federalism should work this way is not entirely clear. So it is for the sake of explaining some of the institutional circumstances conducive to conflict that this essay is written.

THE POLITICAL PARTY AS A SOURCE OF HARMONY OR DISHARMONY

The one agency that might be expected to harmonize the policies of central and constituent governments is a political party. If the officials of both sets of governments are adherents of the same ideology or followers of the same leader or leaders, then they might be expected to pursue harmonious policies. And in the conventional theory of federalism, it is indeed assumed that exactly this will happen. But in all federalisms which have a relatively free party system with competing political parties, just the converse occurs. In the heat of party struggle, competing parties use the central government against the constituent governments and the constituent governments against the central. The only circumstance in which it is perhaps impossible for intergovernmental conflict to appear as a by-product of party competition is when one highly disciplined party controls all governments both central and constituent. This is not, however, likely to occur except in dictatorial federalisms like the Soviet Union or Yugoslavia. The motive for establishing federalisms in all instances (except the present German one and possibly the Yugoslavian) has been to attract the adherents of constituent governments into a larger nation without the difficulties of conquest. By reason of this history, in all federalisms there is a residue of localism and sectionalism, which, given an opportunity to flourish in unrestrained party competition, has always resulted in sectionally based political parties. Even in the Soviet Union, where federalism was introduced under a tyranny, much of the opposition to the single party turned out to be sectionally based and Trotskyism found its chief expression in the government of the Ukraine. And sectionally based party competition, whether in a free or dictatorial party system, is certain to make use of the central government to fight some of the constituent governments and some of the constituent governments to fight other constituent governments and the central government. Hence, the very existence of political parties (or in dictatorships, underground factions) seems in a federalism almost certain to call forth intergovernmental conflicts.

If the existence of sectionally based political parties is, as we have argued, certain to occasion intergovernmental conflict, then the degree of party division among the several governments is a rough measure of the degree of possible conflicts between them. Presumably, if one homogenous political party controlled all governments, both central and constituent, there would be no occasion for intergovernmental conflict. If, on the other hand, all constituent governments were controlled by one homogenous party, and the central government by another the degree of federal conflict would be intense. Between these two extremes lie all existent federalisms. The Soviet Union and Yugoslavia have usually attained the former extreme. On the other hand, the United States, Canada, Brazil, and Australia are at present almost in the center of the scale between the two extremes and have at times approached closer to the latter than the

former. If one could arrange particular federalisms on a scale between these two extremes, then one would have some rough indication of the probable amount, other things being equal, of intergovernmental conflict and lack of integration in policy. Such an arrangement has heretofore been impossible because there was no way to relate the positions of, for example, governor, senator, and president on the same numerical scale. Hence, when, for example, one party controlled the executive and, by a narrow margin, the upper house and when another party controlled the lower house by a wide margin, it was impossible to say which one controlled—or controlled in what degree—a government as a whole. But by reason of some recently developed mathematical techniques, this difficulty has been overcome. And so in this essay, several federalisms will be placed on such a scale.

To obtain an index of disharmony in a federalism, we will use the so-called power index, devised by Shapley and Shubik (9, 10). This index is based on Shapley's theorem on the value of n-person games. The power index is described thus: Assume a legislature of n members in which each member has one vote and in which a simple majority wins. Assume further that the members can vote in any sequence and that any one sequence is as likely to occur as any other. In any legislature, there are $1 \cdot 2 \ldots \cdot n$ such sequences, i.e., $n!$ (read, "n factorial"). The winning voter, that is, the last voter necessary for a majority in the sequence, is called the "pivot." Thus in a five-member legislature with these rules, c is the pivot in the sequence d, a, c, b, e. The power index with respect to any one member (designated here by "P_i") is this simple ratio:

$$P_i = \frac{\text{number of sequences in which } i \text{ pivots}}{\text{number of sequences (that is, } n!)}$$

In the illustration suggested, $P_c = 24/5! = 24/120 = 1/5$.

This index measures the a priori chances that a member of a legislature has to occupy the marginal or "pivot" position in a legislative victory. Assuming that the last added member of a minimal winning coalition[2] can more deeply influence the content of the coalition's legislation than the original members, the index is thus a measure of a legislator's power. Although it was developed on the basis of a simple legislature, one of the attractive features of the index is that it can be adjusted to fit a wide variety of constitutional provisions. Thus, it can be applied to the United States government, which consists of a tricameral legislature in which a majority of those voting in two houses is necessary for the passage of a bill, and, if the majority in either of these is less than ⅔, the assent of the third house is also necessary (7). Following the calculations of Shapley and Shubik, the power indices for these several legislators are: For the President, about .166667; for the Vice-President, about .004295; for each Senator, about .004233; for each Representative, about .000958. Similar calculations can be made for any state government, provided that variations in constitutional structure are allowed for.

For both the state governments and the national government the power indices of those legislators who join a particular political party may be summed. Hence, it is possible to speak not only of the power index of a legislator, but also of the power index of a party. If one could assume perfect party cohesion (i.e., such that, if one member of a party votes on one side of an issue, then all members of that party vote on the same side), then, instead of summing the indices of members of one party, the index of the party itself should be calculated, as if the party itself were a player. Thus, the model would be not a simple majority game, but a weighted majority game. But in American legislatures, both state and national, party cohesion is not extremely high and over a series of roll-calls it is likely that every single

[2] "Minimal winning coalition" is defined as a coalition such that, if one member is subtracted, it ceases to be a winning coalition. In weighted majority games, in which members' votes have different weights, a "minimal winning coalition" is a coalition such that, if the last added member is subtracted, the coalition ceases to be a winning one.

member will defect at least once. The unreality of the assumption of perfect cohesion for American legislatures will be at once recognized when it is pointed out that a calculation on the basis of it would produce an index of one for the majority party. Outside of the legislatures of the deep South this is absurd—and in the South the same results are obtained by summation. Hence it seems appropriate in the United States to obtain party indices by summing the indices of the party members.

Furthermore, if the states are considered as a class together, it is possible to calculate the power index of parties in the system of states taken as a whole. If one regards the states as governments of equal authority, as they are regarded in the construction of the Senate, then the power index of a party in the states taken together is the sum, multiplied by 1/48, of the power indices of all the state legislators and governors that assert they belong to the party. If, on the other hand, one regards the states as governments that vary in authority according to their population, then the power index of a party in the states taken together is the sum of the power indices of that party in the states, each state index having been multiplied by the state's percentage of the national population[3]. Whichever method is used, the resulting index is a rough measure of the strength of a party in the states. Once obtained, it can serve for comparison with the strength of that party in the nation as a whole.

The party division that, by our assumption, disturbs the harmony of federalism is that which occurs when states are controlled by a different party from the one that controls the national government. A convenient index that expresses the degree of partisan disharmony between the two sets of governments is then: For any party (or governing coalition of parties) whose power index in the national government is greater than .5, one minus the power index of that party (or parties) in the sum of the states is the index of partisan disharmony in a federalism. Thus, if the power index of the Republican party in the national government is .63 and in the sum of the states (considering the states as equal) is .54, then the index of partisan disharmony is: $1-.54$, or .46.

[3] In our calculations we have used each state's percentage of members in the House of Representatives, assuming that this percentage is politically more relevant than mere population.

PARTISAN DISHARMONY IN THE UNITED STATES, 1937–56

We have calculated the index of partisan disharmony in the United States for the nine bienniums from 1937 to 1956 (The calculation is not for the biennium as a whole, but only for the beginning of the first legislative session of the biennium in each state and the national government. No attempt was made to take account of subsequent changes by reason of deaths, resignations, changes of party affiliation, and special elections.) The particular period, 1937 to 1956, was chosen, not out of a conviction that the most recent is the most interesting or even the most important, but simply because statistics on the party affiliation of state legislators are readily available for that period alone. Most state governments, subscribing to the eighteenth-century superstition that parties are inventions of Satan, refused to recognize the existence of parties in state publications throughout the nineteenth century and well into the twentieth. Consequently newspapers and broadsides were the only sources for such statistics until the state manuals became common in the first two decades of this century. While the manuals usually notice the party affiliation of elected officials, in a few states still the superstition is adhered to. Since 1937, however, the Council of State Governments has published presumably accurate statistics on party affiliation in *The Book of the States*, and these we have used for our calculations. Doubtless in other periods (e.g., 1877 to 1896), the index would, if calculated, display more interesting vagaries than in 1937 to 1956. But the labor of searching newspaper files seemed less important than the calculation itself; so we used the statistics at hand, intending, if the results seem significant,

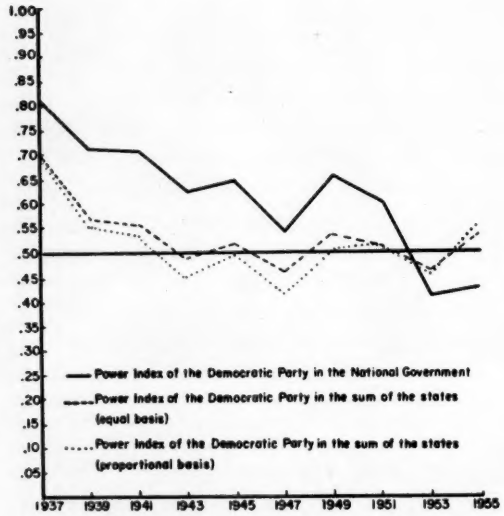

FIG. 1. Power Index, Democratic Party, 1937–55.

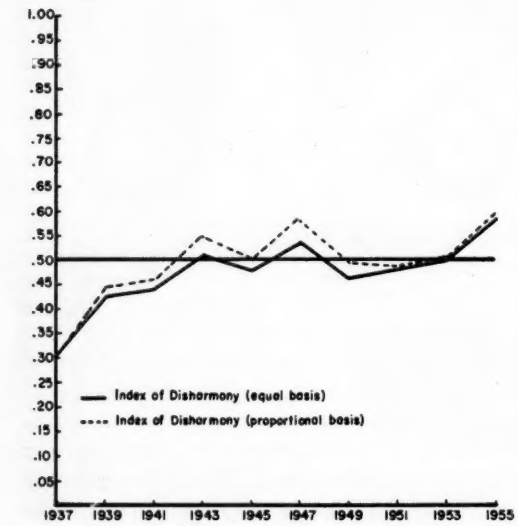

FIG. 2. Index of Disharmony, United States, 1937–55.

to spread the calculations backward in time and more extensively in space.

In calculating the power index of parties in the national government, we used the method described above. In calculating the power index of parties in the states, however, we varied the indices of governors and assemblymen according to the constitutional rules on the governor's veto, the presence or absence of a Lieutenant Governor with tie-breaking powers in the state assembly, the number of state senators and representatives, the number of houses in the state legislature, etc. In the instances of Minnesota and Nebraska we counted all the assemblymen as independents, although, as is well-known, many of them cryptically affiliate with a political party. Finally we calculated the power indices of parties in the sum of the states two ways: (*a*) on the assumption that the states were equal in authority and (*b*) on the assumption that the states varied in authority according to their population. Hence we have two indices of partisan disharmony, one designated "equal basis," the other designated "proportional basis."[4]

The results of the calculation are shown in Figures 1 and 2 and also in Table 1. Figure 1 depicts the power index of the Democratic party in both the national government and the sum of the states. (Inasmuch as independents and affiliates of minor parties are a negligible factor in this period, the power indices of the Republican party are substantially the complements of the Democratic indices.) As is apparent the party in power nationally has in this period done substantially better in national elections than in state elections. We suspect that this has been a characteristic feature of American federalism for a long time, although we do not have the statistics necessary to verify

[4] In most states the constitutional rules on voting are the same as those in the national government. But these states vary in the following details: These states have no Lieutenant Governor: Arizona, Florida, Maine, Maryland, New Hampshire, New Jersey, Oregon, Utah, and West Virginia. And in Massachusetts and Tennessee this officer does not preside over the upper house. In North Carolina the Governor has no veto and in the following states his veto can be overridden by a simple majority (rather than ⅔ majority) either of those present or of those elected: Alabama (except for 1945–49), Arkansas, Connecticut, Indiana, Kentucky, New Jersey (up to 1949), Tennessee, and West Virginia. In Delaware, Maryland, Nebraska, Ohio, and Rhode Island the Governor's veto can be overridden by a ⅗ majority of those present or of those elected. (It was found impossible in the construction of the index to draw a distinction between a requirement involving those present and one involving those elected.)

our assumption. It follows from the pattern of indices for the Democratic party (Figure 1), that the index of partisan disharmony (Figure 2) is fairly high. Except for 1937–38 when it was .30, this index has varied between .40 and .60. Interpreting the index as an indication of a serious degree of disharmony when it rises above .50—the point at which the party in control of the national government fails to control the states taken in sum—then roughly half the time in recent years, American federalism has experienced a serious amount of disharmony. If the index is calculated on an equal basis, then in four bienniums out of ten it has risen above .50; if calculated on a proportional basis, there have been five such bienniums out of ten.

ASSUMPTIONS OF THE INDEX

If the assumptions beneath the index of disharmony are valid, the foregoing calculations reveal a disturbingly serious—and, we believe, heretofore unsuspected—disability in American federalism. But before we can examine this disability, we must first examine the assumptions of the index. Some readers will doubtless feel that an index so contrived and so abstract is impossible to interpret and hence of no value in the study of constitutions. Counting, we must all admit, destroys the humane richness of unordered detail. But numerical summaries, although they drown the uniqueness of individual facts, do permit us to comprehend detail that is otherwise beyond our grasp. We count in order to comprehend; yet of every essay in counting we must ask whether or not the counting itself abstracts so far from reality that it precludes comprehension of the things counted. Certainly that question is appropriate here; and so, before proceeding with our main business, it seems proper to say a word about the validity of the assumptions of the index.

The most obvious of the limitations of the index is that it measures unhomogenous things. It measures the division of power (defined as the chance of pivoting on roll calls) among political parties in legislatures. Yet in the United States, the members of a political party are not a homogenous class with respect to their attitudes on public questions nor with respect to their actions in legislatures. Not only is party cohesion rather low in American legislatures; but further, taking a party as a national organization, many members may be diametrically opposed on the public issues of the day. So great is the difference between, for example, a negro Democratic assemblyman from Detroit, Michigan, and a white Democratic assemblyman from Montgomery, Alabama, that a category subsuming both of them may be utterly meaningless. The assumption of the index is that, if the Democratic party, e.g., controls the national government, the Republicans will use the state governments they control to impede the center, while Democratic governments in state and nation will work harmoniously together. It may well be, however, that a dissident Democratic faction in control of a state will obstruct national action far more seriously than Republicans and that the national Democrats will work quite harmoniously with a dissident Republican faction in control of another state. In the United States, where shades of political opinion are expressed in factions rather than in parties, the possibility is considerable of actual situations in violation of the assumption. Such situations are less likely in multi-party systems where shades of opinion tend to be expressed in parties, although in New South Wales in the 1930's the Lang Labour Party seems to have occasioned exactly this sort of factional division.

And yet this limitation of the index is not nearly so serious as it appears to be. All factions of a party, no matter how bitter their squabbles, are agreed at least on the preservation of the party itself. The fact that they continue to associate themselves with it sufficiently indicates that, e.g., even the most disaffected Republicans would rather be Republicans than Democrats. In short, intraparty squabbles are moderated by the understood compact to preserve the party in a way that conflicts between parties are not. And in that sense the assumption beneath the index of disharmony is fully justified. The justification can perhaps be realized more vividly if one

imagines an index of disharmony calculated for factions rather than parties. Assuming that there were some valid empirical device to identify the factions around which such an index would be constructed, still an index of factional (rather than partisan) disharmony would necessarily involve the assumption that factions are durable and coherently organized. This assumption is even more questionable, with respect to American politics as it is usually described, than the assumption that parties are factionless. It seems, therefore, that an index of partisan disharmony, while not a wholly adequate model of American politics, is yet the best that can now be constructed. The reliability of a model constructed with the perhaps unreasonable assumption that factionalism is less obstructive than partisanship can only be discovered by an empirical test of the results of calculations with it.

A second possible limitation in the index is that in the states taken together all the power of all members of a party is counted, whether the party is in control of the state government or not. Thus conceivably the index of disharmony would be as high as .49 even though one party had a power index greater than .50 in the national government and every state government. In the United States, with its 48 constituent units, such considerable distortion is barely conceivable for the minorities in the states tend to cancel out. But in the federalisms with fewer constituent units, e.g., Canada and Australia, distortion of this sort is less unlikely.

And yet it is not certain that this is in fact a statistical distortion. If the majority party has everywhere only a tiny margin of control, the expectation of factional disaffection is greatly increased. Such factional defection may well lead to using the governments of a federalism against each other—and this is what the index of disharmony is supposed to measure. In the United States in the last two decades, the dissatisfaction of the conservative Southern faction with the liberal Democrats of North and South has seemed to increase as the amount of Republican opposition has increased[5]. Presumably, when the margin between the two parties is narrow, the conservative faction has believed it could successfully carry its disputes much further than when the Democratic party could win without them. It is not at all inconceivable, therefore, that the amount of actual disharmony increases when the index increases, even though the minority party nowhere controls a government. If this is in fact true, then this apparent defect in the index is actually meritorious. Whether it is the one or the other, however, depends upon the result of some kind of empirical test.

A third limitation of the index of disharmony arises from the nature of the power index. In the construction of the latter the notion of power is defined as the chance a legislator has of pivoting in a roll-call. It is assumed that legislative rewards, i.e., influence over public policy, accrue in greatest quantity to the marginal legislator. Power in the ordinary sense of the word is, however, much more than the power index allows. It is a vast complex of the institutional and personal ways to influence public policy and governmental action. These ways of influence, power in the ordinary sense, may or may not be related to the chance of pivoting on a roll-call. If they are not related, then the greatest rewards may accrue, not to the marginal legislator, but to the most advantageously situated. E.g., the shape of legislation may more faithfully reflect the opinions of an energetic, persuasive and senior committee member of the minority party than the opinions of any other legislator. And if power in the technical sense of the power index is not in nature closely related to power in the ordinary sense, then the power index may not measure anything at all in natural governments. It does seem likely, of course, that the chance to pivot is a basic sort of power out of which grows nearly every other kind of influence

[5] Conservative Southern dissatisfaction has, of course, declined when the Democratic party has ceased to be the majority party in the national government. The observation in the text is limited to the period in which the Democratic party possessed a national majority.

in a representative government. Yet, in the absence of a reasonably impressive empirical test, one cannot be certain that the power index and hence the index of disharmony serves as a model of anything in real governments.

Whether or not these three limitations are extensive enough to destroy the interpretive value of the index of disharmony depends in each instance on some kind of empirical evidence. We have, therefore, sought for some empirical test that would indicate whether or not variations in the index are connected with variations in actual conflict between governments. Such a test is not, however, easily devised. For one reason, federal conflict seldom consists of commensurable events. It is hardly possible to measure on one scale both a civil war and a dispute over the routing of a highway. For another reason, many petty disputes are never publicly reported and are for that reason uncountable. One kind of federal conflict, i.e., formal litigation in courts, is, however, sufficiently recurrent, sufficiently reported, and, we trust, sufficiently commensurable to serve as an empirical indication of disharmony. And so we have constructed a test around it.

We assumed that, when the nation and the states carried their lawsuits as far as the Supreme Court, some actual disharmony existed. In many kinds of litigation between the two sets of governments, lawsuits are a means of amicable settlement of questions over which there is no serious disagreement. When, for instance, the United States acquires land owned by a state, it must pay something for it and one way to arrive at an impartially established price is by condemnation proceedings. If the price set by the district court is accepted by both parties, one can assume that the two governments were simply using the court system as a substitute for the market mechanism. But if land condemnation proceedings are carried up through the court of appeals to the Supreme Court, one can assume that the nation and the state have disagreed seriously either about the transfer or the price. So it is with most other types of federal litigation that reaches the Supreme Court. From a survey of all the cases summarized in columns I and II of Table 1, it appears that all but two, or perhaps three,

TABLE 1

CASES CONSIDERED BY THE SUPREME COURT IN WHICH UNITED STATES AND STATE(S) WERE BOTH INVOLVED[a,b]

Biennium	Col. I Cases in which nation and state(s) opposed[c]	Col. II Cases in which nation and state(s) aligned[d]	Col. III Net cases in which opposed Col. I less Col. II	Col. IV Index of disharmony (equal basis)	Col. V Index of disharmony (proportional basis)
1937–38	21	13	8	.30	.31
1939–40	29	14	15	.43	.44
1941–42	33	12	21	.44	.46
1943–44	47	14	33	.51	.55
1945–46	24	7	17	.49	.51
1947–48	27	5	22	.54	.58
1949–50	24	6	18	.46	.50
1951–52	30	9	21	.49	.49
1953–54				.50	.51
1955–56				.58	.60

Note.—$r_{\text{Col. III. Col. IV}} = .789$; $r_{\text{Col. III. Col. V}} = .786$

[a] "Cases considered by the Supreme Court . . ." means not only those cases in which the Court heard argument and issued a decision, but also all those cases in which a prayer for a writ of certiorari was refused or an appeal was not granted.

[b] Cases in which both the United States and a state(s) were litigants were assigned to the biennium in which the suit was instituted, whether in a lower court or in the Supreme Court. Thus a case decided in 1947 might be counted as much earlier as 1942. (This is why it has been impossible to carry the correlation further than 1951–52). Cases appealed from an administrative tribunal were assigned to the biennium in which the decision of the tribunal was appealed to the courts. Cases in which either the United States or state(s) appear only as *amicus curiae* were assigned to the biennium in which the brief was submitted. Cases in which proceedings were instituted or briefs were submitted in the first two weeks of January were assigned to the previous year.

[c] Column I summarizes (*a*) those cases in which the United States or its instrumentality or agent was a litigant against a state(s) or its instrumentality or agent; (*b*) those cases in which, when a state(s) was a litigant, the United States submitted a brief *amicus curiae* against the interest of the state(s); and (*c*) those cases in which, when the United States was a litigant, a state(s) submitted a brief *amicus curiae* against the interest of the United States.

[d] Column II summarizes (*a*) those cases in which the United States or its instrumentalities or agents were litigants on the same side of a case as a state(s) or its instrumentalities or agents; (*b*) those cases in which, when a state(s) was a litigant, the United States submitted a brief *amicus curiae* supporting the interest of the state(s). In those special cases in which the litigants were two instrumentalities or officers of a state government and in which the United States intervened either as a litigant or as *amicus curiae*, we counted the case under Column II if the United States intervened on the side of an elected official or if it intervened on the side with the most elected officials. Otherwise, we counted it under Column I.

involved a genuine controversy between the nation and the states. (The exceptions were cases in which a state was a litigant and in which the Solicitor General of the United States submitted, at the request of the Court and apparently with reluctance, a brief *amicus curiae*.)

Assuming then that federal cases in the Supreme Court reveal the existence of actual federal disharmony, we counted the number of such events and correlated this number, by bienniums, with the index of disharmony. This calculation resulted in a coefficient of correlation of about +.79. Testing this coefficient against the hypothesis that it might have occurred by chance, it appears that the coefficient is just within the 2 per cent level of confidence, which is to say that a correlation this high would occur by chance only in 2/100 of the possible correlations. The coefficient of .79 can hardly be a chance result and we are, therefore, justified in concluding that there is some positive relationship between the index of disharmony and actual disharmony in American federalism.

High correlations, even correlations within the 2 per cent level of confidence are, admittedly, not especially impressive when only eight items are involved. Still there is a good, non-statistical reason for believing that the correlation is more impressive than it appears. A large portion of the cases between the nation and the states before the Supreme Court stand as part of persisting and durable controversies not likely to be affected by biennial changes in the relative position of parties. The controversy over state taxation of Indians and Indian lands, the jockeying for control of the natural gas pipeline business between the Federal Power Commission and state utility commissions, the controversy over reciprocal immunity in taxation—these and similarly durable controversies account for about one-third of the cases in Table 1. Were they eliminated, the variation in the number of cases per biennium would be even more volatile than it is and a much higher coefficient of correlation would result.

It seems fairly certain, therefore, that changes in the index of disharmony are accompanied by positively correlated changes in the actual amount of disharmony as expressed in litigation before the Supreme Court. Unfortunately this positive correlation is not a full test of the reliability of the index. Formally and legally, litigation is instituted by executives alone and in their executive capacity, while executives in their legislative capacity account for less than one-sixth of the total power measured in the index. Still it seems likely that the amount of actual disharmony is dependent upon the political atmosphere of the moment, that, for example, executives find it necessary to institute lawsuits against other governments especially when there is a high degree of partisan disharmony in all branches of government. And if this kind of political atmosphere is important in actual disharmony, then the test, for all its lop-sided emphasis on executive action, is still closely related to the index.

Indeed, in light of the test, we are justified in concluding that the index is at least partially reliable (*a*) even though it ignores factions within parties; (*b*) even though it ignores the fact that some of the power measured is held by minorities; and (*c*) even though the power index on which it is based assumes a very special definition of power. Undoubtedly many individual instances can be discovered in which power in the ordinary sense is quite unrelated to the chance of pivoting in roll-calls. The fact that a fairly high correlation is obtained between the index of disharmony and actual disharmony suggests, however, that the chance of pivoting is at least an adequate sample of power (in the sense of influence on public policy) in natural legislatures. Thus indirectly—and doubtless tentatively—this test serves also as a test of the validity of the notion of the power index.

Thus, in spite of these limitations, the test shows that to some degree the index of disharmony measures what it purports to measure. The explanation of this possibly curious situation is, we suspect, analogous to the explanation of similar situations in economics. Even though the measure is crude, the fact that it subsumes a vast amount of detail permits conflicting data to cancel out

so that we are left with a general statement which, though oversimplified, reliably expresses a gross relationship.

APPLICATION OF THE INDEX TO OTHER GOVERNMENTS

The index indicates, as we have suggested, the existence of a serious amount of actual disharmony in American federalism. Just how serious this defect is, however, can hardly be comprehended until the index for the past twenty years in the United States is compared with the index in the United States at earlier periods and with the index for other contemporary federalisms. It would be especially interesting, for example, to discover what effect the passage of the seventeenth amendment had on the index of disharmony. There are good a priori reasons for believing that this amendment tended to increase the index; whether or not it actually did so, however, can be determined only after further data for the calculation of the index are collected. For the reasons previously set forth, we will not here attempt to calculate an index for the United States prior to 1937; but sufficient data are at hand to calculate an index of disharmony for Australia and Canada for the same period as it was calculated for the United States[6].

By contrast with the index of disharmony for the United States, the indices for Canada and Australia are startling. The Australian index shows extreme variation and extreme disharmony both when calculated on an equal basis and when calculated on a proportional basis. The Canadian index, when calculated on an equal basis, shows less extreme variation than the Australian, and is, except for 1940 to 1944 not vastly different from the two American indices. But when calculated on a proportional basis the Canadian index shows a greater variation than any other (from .09 to .86).

This is an extraordinary result, the more so since for both Canada and Australia it seems reasonable to regard the index constructed on a proportional basis as the more appropriate of the two. In the structure and operation of the United States Senate, the states are theoretically and practically regarded as equal in significance, while in the House of Representatives they are regarded as significant in proportion to population. The Senate also has the same amount of legislative power (in the technical sense) as the House and in addition some so-called executive power the House lacks. Hence it is reasonable, and probably necessary, to calculate the index of disharmony on both bases. In Canada, however, both the Senate and the House of Commons are constructed roughly in proportion to population (the Senate less so, for it is rather heavily biased in favor of the Maritime provinces and against the western provinces). In addition, the Canadian Senate is wholly powerless (in both the technical and popular senses). Hence, an index of disharmony on an equal basis hardly represents the constitutional relationships among the provinces and the center; and one ought probably to regard the index on a proportional basis, as the only appropriate measure of federal disharmony in Canada.

[6] In the calculation of the index for these countries, an important change must be made in the method of calculation. In Australia, the cohesion of parties on roll calls is said to be exceptionally close (cf. Ref. 4, p. 92), although we know of no statistical studies of party discipline. In Canada, likewise, party cohesion is said to be quite close (cf. Ref. 2, p. 241 ff.), although again we know of no statistical studies. When party cohesion in a legislature is high, then the model of a simple majority game is no longer appropriate. The legislature is more realistically interpreted as a game played, not by individuals, but by parties with unequal voting power. Hence the appropriate model is a weighted majority game; and we have thus calculated the power indices for Canadian and Australian parties. (For the method of calculation, see the section by A. W. Tucker in Ref. 1). The change of method has this significance: If one party has an absolute majority of the legislature, then it has all the power and an index of one. If a party has less than an absolute majority, its index is less than one, but often quite disproportionate to its membership. In general, the largest and smallest parties have more power per member than their members would have in a simple majority game, and the medium-sized parties less. The effect of this mathematical characteristic is to occasion more violent fluctuations of the index of disharmony than were found in the United States; but as most students of Canadian and Australian federalism would agree, this intensification of the movement of the index faithfully represents the temper of these constitutions.

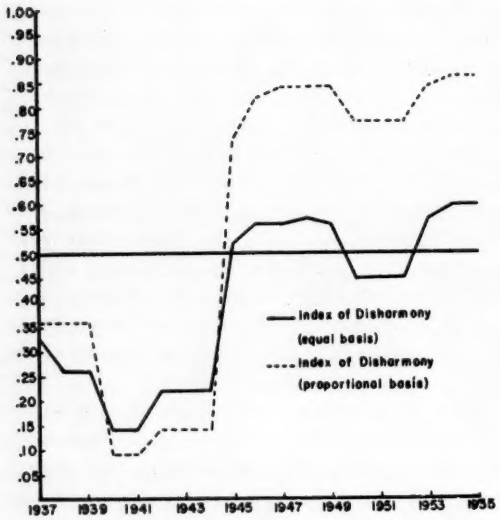

FIG. 3. Index of Disharmony, Canada, 1937–55.

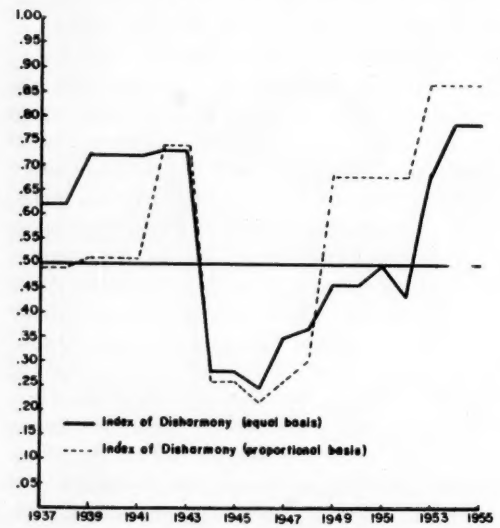

FIG. 4. Index of Disharmony, Australia, 1937–55.

In Australia, the Senate is constructed on an equal basis and has somewhat more legislative power (in the popular sense) than the Canadian Senate; but since the Australian Senate is far less important than the House of Representatives, the index constructed on a proportional basis seems the only appropriate measure for Australia as well. Inasmuch as the index on a proportional basis displays for both countries a far greater variation and much more disharmony than the index constructed on an equal basis, Figures 3 and 4 reveal an even more startling result than might at first be assumed.

The extraordinary indices for Canada and Australia suggest that these two federalisms are plagued with much more federal conflict than is the United States. They suggest strongly that constitution-makers in both countries were unwise to copy American federalism as slavishly as they did and that they might more wisely have adjusted federalism to fit parliamentary as distinct from presidential government. Federal conflicts are disconcerting enough in the United States. If the index is a valid representation of Canadian and Australian reality, such conflicts must be far more disconcerting in these two countries.

Whether or not the index of disharmony validly represents political events in Canada and Australia, we are unable to say. We are quite certain, however, that the extreme variation in the Australian and Canadian indices is not an accident of the method of computation. It is true that the use of the weighted majority game model is likely to represent the power of a party as much higher or much lower than the popular vote might suggest. Using the weighted majority model, if the party having a national majority wins in every state, the index of disharmony will be zero, even if it wins in each state by only 51 per cent of the vote. But when party cohesion is very high, as in Australia or Canada, this is a fair statement of the political situation: Power is *not* divided among the parties in proportion to the popular vote, but all of it accrues to the winner. That the two kinds of models (weighted majority and simple majority) have here been appropriately applied is well demonstrated by comparing Republicans' very good chances of pivoting in Kentucky with Liberals' very poor chances of pivoting in Queensland or Quebec. However, we leave it to those more versed than we in the details of the operation of these two governments to devise a test of our index. (It seems possible that for Australia a test similar to the one here used for the United States might be devised around the operation of the Commonwealth Grants Com-

mission.) Although we lack a satisfactory test, there are some subjective indications that federalism occasions much greater difficulty in the Commonwealth and the Dominion than in the United States. In *Federal Government*, Professor Wheare remarks (apparently with some surprise) that in "Canada alone of the federations has there been conducted a thorough investigation of the whole working of the federal system" (11). If the indices are a valid representation of the degree of disharmony in the three federalisms, however, it is not at all surprising that Canada, in which the highest degree of disharmony is indicated, should have conducted an elaborate inquiry into federal relations. The Canadian Royal Commission on Dominion-Provincial Relations, which reported in 1937, did not, however, consider abandoning federalism. On the other hand, the minority report of the Australian Royal Commission on the Constitution, which reported in 1929 and which, despite Professor Wheare, conducted a study fully as elaborate although less academic than the Canadian one, did quite seriously propose to abandon federalism. (5) One can hardly imagine the proposal of so fundamental a revision unless some Australians believed that much federal disharmony occurred. Since Professor Wheare's writing, the United States has also conducted a study of federalism (by the Commission on Inter-governmental Relations). Unlike the Canadian and Australian commissions, however, its studies raised no fundamental questions for they were concerned with techniques rather than public law, its members lacked importance in national politics, its recommendations were hardly noticed by Congress or the press, and its reports created almost no public interest. The contrast between the work and significance of the Canadian and Australian commissions on the one hand and the American on the other clearly suggests that federalism occasions much more difficult problems of governing in Canada and Australia than in the United States—which is what the indices suggest also.

Why federalism should have so much more debilitating effects in one place than in another is not entirely clear. Much more detailed empirical and comparative study is needed before this question can be satisfactorily answered. One possible answer that occurs to us, however, is that federalism operates quite differently in two-party and more than two-party systems. Another is that federalism operates quite differently when the number of states is small and when it is large. A third is that federalism operates quite differently in parliamentary and presidential systems.

As we observed earlier, political parties in a federalism are likely to be geographically based, each one controlling some constituent governments. If there are more than two parties controlling constituent governments, then the likelihood is decreased that the party or coalition with a national majority will have a majority in the sum of the states. In Canada, for example, where there are presently five major parties each with a geographic base (Liberal in the Maritime Provinces and Ontario, Union Nationale in Quebec, Conservative in Ontario and parts of the West, Cooperative Commonwealth Federation in Saskatchewan and British Columbia, Social Credit in British Columbia and Alberta), it is hardly possible that any party have a majority in the sum of the ten provinces. Although we have no statistical proof of this assertion, it seems likely that the more parties in existence, the greater the likelihood of federal disharmony.

In Canada with ten provinces and in Australia with six states, the number of constituent governments is much smaller than in the United States. Here, the party with a national majority can lose control of as many as eight or ten states without altering the index of disharmony as much as +.10. In Australia and Canada, however, if the party with a national majority loses Victoria and New South Wales or Quebec and Ontario (both pairs of which constitute well over half the sum of the states or provinces on a proportional basis), the index can change as much as +.60. And, if the index does indicate actual disharmony, this is not a mere numerical change, but represents a sharp reversal in federal relations and the possibility of serious discord between the two largest constituent govern-

ments and the center. In short, when the number of states is small, a very few elections can dramatically alter the index, thus leading to an instability not likely to occur in a federalism with numerous constituent units.

In a presidential system like the United States, it is entirely possible that national and state elections be held simultaneously. Since 1872, Presidential electors and Representatives in Congress have been elected at the same time as governors and assemblymen; and since 1913, Senators have been too. Except for some elections midway in the term of a President, voters are thus forced to decide national and state issues simultaneously and, not unnaturally, they have tended to decide them by plumping for one party. This tendency has kept the index of disharmony below .50 in all the bienniums (1936–54) following a Presidential election, although it has often risen above .50 in bienniums preceding Presidential elections. In Canada and Australia, however, central and constituent government elections have, so far as we know, never occurred simultaneously. To require that they do so would seriously disturb the theory (although not the practice) of parliamentary government, in which dissolutions can occur at any time. Consequently, voters are able to judge local and national issues by entirely different standards. Thus the voters of Quebec can consistently elect an anti-Liberal provincial government while yet returning Liberals to Ottawa. In Australia the extreme variations in the index result from the fact that, in the period under consideration, the Labour party has usually controlled the sum of the states. Hence the index is low when the Labour party controls the Commonwealth, and high when it does not. In Canada, however, the Liberal party has controlled the Dominion government throughout the entire period under consideration. All the extreme variations in the index result from voters making different judgments on the Liberal party in state and national elections held at different times.

All three of these institutional alternatives (two or more parties, few or many states, simultaneous or temporally separate state and national elections) may thus affect the variations and level of the index. And indeed it is quite possible that other institutional alternatives that have escaped our notice may also affect it considerably. In the absence of detailed empirical evidence, we cannot say what the relationship among these alternatives in their effect on the index may be. It seems likely, however, that a federalism with more than two significant parties, relatively few states and a parliamentary government, is almost certain to experience severe federal disharmony, especially if the constituent governments have anything important to do. The government of West Germany is just such a federalism. Although we have not calculated an index for this government, it appears from casual inspection that the Christian Democratic party has had a majority at Bonn and in the sum of the Länder. There are signs, however, that the majority in the Länder is now slipping away and hence it seems likely that this government will also soon find federalism as uncomfortable as it has been in Canada and Australia. From a German point of view at least, it is unfortunate that the German basic law was so blithely fashioned as a federalism without consideration of the potential disharmony to be faced. Perhaps, if West and East Germany are ever united, German constitution writers will have a second chance.

Even one minor reform might mitigate disharmony in federalisms of the sort here discussed: a provision that all legislatures in constituent governments be elected at the same time as the national legislature. In a parliamentary government, this proviso would require, of course, that a dissolution of the national legislature automatically entail a dissolution of all state legislatures. Lest dissolution in one state force a dissolution on the nation, state legislatures would, of course, be restrained from unseating a state ministry.

CONCLUSION

In comparison with Canada and Australia the index of federal disharmony in the United States has been neither as high nor as low.

In the period under consideration, it has, with one exception, ranged in the area of .50 ± .10. American federalism might thus be described as consistently disharmonious at this level. While it is not disturbed by an index in the range of .80 to .90, it does not experience an index as low as .09. The Canadian and Australian governments, in their occasional periods of proximate harmony are probably able to bring about necessary readjustments in the federal relation. In the United States, however, such opportunities are probably less frequent. Hence it may well be that, although the index never rises as high in the United States as elsewhere, the effect of consistent disharmony at the level of .50 ± .10 is fully as debilitating as occasional disharmony at the level of .88. It is apparent, however, that before we can discuss the effect of this consistent disharmony we need to project the index farther backward in time, perhaps as far back as the first appearance of the modern party system in the 1830's.

This essay was undertaken in order to explain some of the institutional circumstances conducive to intergovernmental conflict. We have shown that partisan disharmony between central and constituent governments is (in the United States) related in quantity to one kind of actual conflict and we have set forth what we believe are sufficient reasons for assuming that partisan disharmony as indicated by the index is at the root of most intergovernmental conflict.

For this reason the index of disharmony has significance beyond the actual measurement it makes. Most American students of public law have in recent years tended to ignore or gloss over the amount of actual conflict in American and other federalisms. Perhaps they have unconsciously assumed that, since our federalism is apparently permanent, there is little point in examining its possible inadequacy. (Such an attitude involves, however, the abandonment of the critical function of scholarship and is, of course, ultimately a disservice to the institutions vacuously praised.) We think it likely, however, that the crucial reason our federalism is discussed in unrealistic terms is that political scientists have lacked a precise vocabulary to talk about conflict and disharmony. Still today, the predominant technique in the discussion of federalism is that of speculative philosophy, and juristic arguments devised in the 1780's are still seriously advanced as *descriptions* of federalism.

It is our hope that, by offering this index, some of the discussion may be rendered more precise. For example, in the hands of Professor Wheare, the definition and justification of federalism is reduced to an epigrammatic paradox when he says that federal government "stands for multiplicity in unity." The existence of a paradox is a sufficient indication of imprecise words that have no place in science or philosophy. If this sentence is, however, formulated in terms of the index of disharmony (e.g. "federal government in the United States in recent years has stood for disharmony at the level of .50 ± .10."), then the connotative approbation in the epigram is removed. Whether or not such a level of disharmony is desirable is still a legitimate question for empirical and interpretative inquiry, but the conclusion is at least not forced by the premises. More importantly, however, it is possible to speak precisely of just what unity and multiplicity are (i.e., an index of zero and an index of higher than zero). Once one can speak precisely, it is then at least possible that the vague mist heretofore shrouding discussions of the federal relation may be lifted.

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(Manuscript received January 24, 1957.)

The most persistent error of modern educators and moralists is the assumption that our social difficulties are due to the failure of the social sciences to keep pace with the physical sciences which have created our technological civilization. The invariable implication of this assumption is that, with a little more time, a little more adequate moral and social pedagogy and a generally higher development of human intelligence, our social problems will approach solution. . . . They do not see that the limitations of the human imagination, the easy subservience of reason to prejudice and passion, and the consequent persistence of irrational egoism, particularly in group behavior, make social conflict an inevitability in human history, probably to its very end.

—REINHOLD NIEBUHR

THE ACQUISITION AND APPLICATION OF INFORMATION IN THE PROBLEM-SOLVING PROCESS:

An Electronically Operated Logical Test[1]

by Erwin Roy John and James G. Miller

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Brothers and sisters have I none
But this man's father is my father's son.

If a man has only ten trees and wants to plant them in five rows with four trees to a row, how can he do it?

1, 2, 3, 5, 7, 11,—,—

How did you solve these problems? What mental processes did you use? How can these processes be measured? With the advent of electronics, "thinking machines," and information theory has come the possibility that human problem solving can now be studied in a more objective, quantitative, and systematic way.

BECAUSE of the importance of cognition or thinking in human life, and particularly because of its significance in productive and creative activity, the study of human problem solving has always fascinated psychologists. In spite of this great interest, progress in attaining an understanding of the problem-solving process has been slow and arduous. One source of the difficulty has been the concern with the achievement, or nonachievement, of a product of the problem-solving process—the solution to a problem—rather than with the process itself. In order to study problem solving analytically, it is desirable to relate the product to the process which produces it. Problems may be solved using different processes, and failures to solve a problem may have many explanations. The major reason for emphasis on the product is that we have not known how to observe and measure the process; we have had methodological difficulty in externalizing the process in an operational way. Attempts to understand the process have relied on the introspection of subjects who have completed a task, or on the verbalizations of subjects while working on a problem. Psychologists working on problem solving have for a long time wanted to find a method for externalizing process in an objective and quantifiable way (1). Although some advances have been made recently, they are limited because the problems have been discrete technical problems, such as the trouble shooting of electronic military gear (2,3) or problems of medical diagnosis (8), and the results have been difficult to generalize.

For greatest utility and potentiality, a technique for the study of human problem solving must meet several criteria: The content of the problem itself should require minimum technical or previously achieved knowledge; the complexity of the problems should be quantifiable and variable; the effects of learning should be minimal from problem to problem; a number of problems of

[1] Some of the work included in this paper was done in connection with research supported by the Office of the United States Army Surgeon General under Contracts No. DA-49-007-MD-575 and DA-49-007-MD-684, Dr. James G. Miller, Principal Investigator. The opinions expressed in this publication are not necessarily those of the Office of the Surgeon General, Department of the Army.

comparable complexity should be available so that a series of comparable measures may be obtained; and most important, the method should permit the externalization and quantitative measurement of the problem-solving process along continua of dimensionality.

This paper presents a technique that meets the above requirements and some results obtained with this technique.[2]

THE PSI APPARATUS

No matter what the specific nature of a problem may be, all problems have two general characteristics. Problems can be resolved into elements, that is, the specific components involved, and into relationships which are the ordering of these elements. Knowledge of elements and the ability to manipulate this knowledge in accordance with the constraints imposed by the relationships between them are the two requirements for the solution of problems. Relationships and the implications of relationships are the concern of logic. Symbolic logic treats of the formalization of relationships between abstract elements. The relationships that are used in symbolic logic can be achieved in automata, as was recognized by Shannon (9). McCulloch and Pitts (6) have proposed a model of a neural net whose structure is a reflection of the logical propositions describing the activity of the net. They have shown that for any consistent set of such propositions a net can be constructed. This theory of automata has been further developed by von Neumann (10).

An apparatus to measure *Problem Solving using Information*, called briefly the *PSI apparatus*, has been constructed that permits the combination of a number of elements into various logical relationships. The elements are represented by invariant electronic connections which in the latest version are transistors built into the apparatus. The logical relationships between these elements can be varied by means of a plugboard in the rear of the device. A set of logical relationships between the elements constitutes a problem. The connections determining a particular problem are wired on to a plug which is inserted into the plugboard for each problem.

Photographs of the apparatus are shown in Figures 1 and 2. On the panel is a circular array of lights with corresponding pushbuttons. Each light represents an element and, depending on whether it is on or off, the state of the element. In the center of the panel is a light with no pushbutton, which represents the output of the circular array. A disc that can be inserted in the center of the array has arrows drawn on it which show the relationships between the elements. For each problem there is a different disc. All relationships are represented by arrows, and each depicted arrow stands for an existent relationship. The relationships possible are conjunction, disjunction, and negation, (hence also implication). The direction of relationships is indicated by the head of the arrows. The arrows indicate only the existence and the direction of the relationship, not the kind of relationship. For example, the arrow between lights V and II might mean (*a*) that if V is lit, then II will light, or (*b*) that to light II, it is necessary but not sufficient to light V, or (*c*) that V prevents the lighting of II. If there is no arrow between two lights, there is no relationship between them; in other words, the null relationship is not presented.

In all problems the task is the same. The central light, representing the output of the network of elements, must be turned on by some combination or sequence of activation

[2] This research was carried out in the Committee on Behavioral Sciences of the University of Chicago. The authors wish to acknowledge their indebtedness to a number of people who participated in various phases of this work. The PSI apparatus was devised in collaboration with Dr. Horatio J. A. Rimoldi, now of the Department of Psychology, Loyola University, Chicago. Dr. Rimoldi also participated in the gathering of the data for the Ph.D. population. The PSI apparatus was designed by Mr. Stanley Molner, now at Marchant Research Corp., Oakland, Calif., in collaboration with the authors. The mode of analysis and scoring of PSI performances which is presented was developed with the help of Mr. Martin Balaban, of the Psychology Department of the University of Chicago, and Mrs. Margaret Labadie. The analysis of the data relating PSI performance to various personality measures was performed by Mr. Sidney Blatt, of the Psychology Department of the University of Chicago, who contributed in addition many ideas of value in the course of the research.

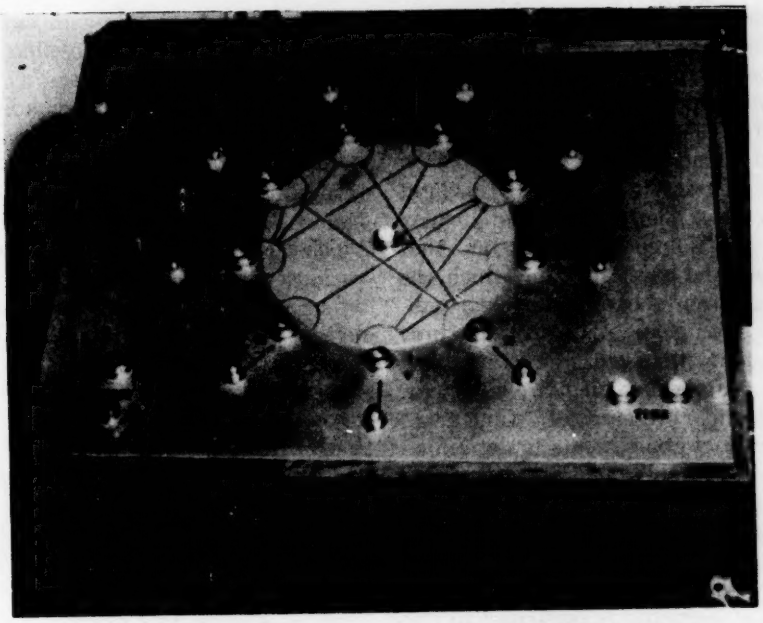

FIG. 1. Front of PSI Apparatus, with Example Disc in Place.

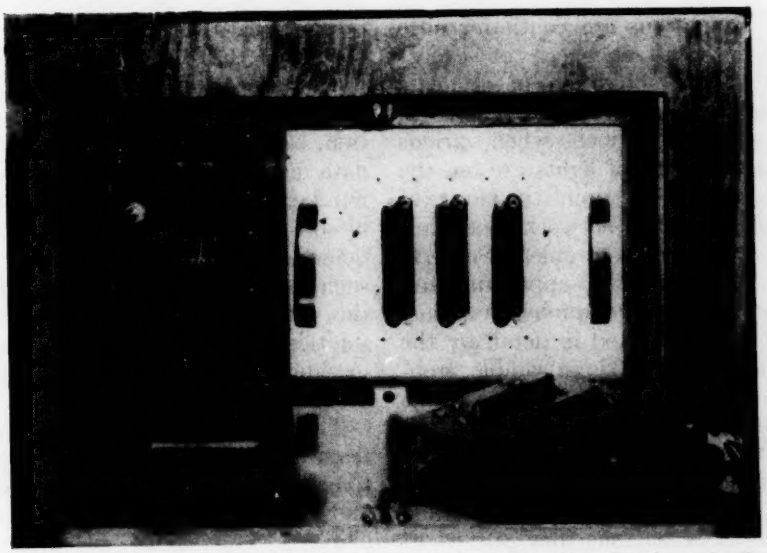

FIG. 2. Rear of PSI Apparatus, Showing Plugboard and Problem Ready to be Inserted.

of three particular elements, which are referred to as input elements. Any element or combination of elements can be activated by depressing the appropriate pushbuttons. In order to learn how to produce the output of the network—to solve the problem—the subject must analyze the relationships that exist between the input elements, the intervening network, and the output. This analysis is carried out by designing small-scale logical "experiments" on the apparatus, which are performed by activating the appropriate elements at the proper time. The subject is permitted to use *all* the elements as often as he wishes, and to construct as many "experiments" as he may find

necessary in the course of working on a particular problem. "Solution" is achieved when he has learned how reproducibly to produce the output by some sequence or combination of activation of *only* the permitted input elements.

When an element is activated, the light representing that element on the display panel goes on. Three seconds later, that light goes out and the lights for which the preceding light represents a sufficient condition for activation go on, remain on for three seconds, and in turn go out, activating those elements for which they are sufficient. (A three-second presentation time was selected after pilot experiments to determine the stimulus duration which afforded ample time for observation and occasioned minimum annoyance because of enforced waiting for results.) In this way, an ordered set of consequences of the activation of any combination of elements is presented to the subject. The subject is free to add further activated elements while the consequences of previous activations are continuing. He may thus observe sequences of events resulting from the activation of elements and the interaction of multiple sequences. By trying out what happens when various lights or combinations of lights are on, the subject can uniquely determine the nature of all relationships in the network, and thus find out how to solve the problem. Subjects become familiar with the apparatus and situation by means of comprehensive illustrative problems presented in detail by the experimenter, and solve these sample problems before measurement begins. Average time for familiarization and testing on the two problems used here is about two hours. In gathering the data here reported, subjects were asked to minimize both the time and the number of buttons they pushed to reach a solution.

The raw material for our analysis of the problem-solving process is the sequence of experiments that are performed—that is, the sequences of buttons pushed and the time at which they are pushed. These data are recorded automatically on tape, which can then be examined and analyzed along many dimensions.

The information content of the network representing any problem can be determined in bits. With the possible exception of the progressive matrices test (7) this is the only test of mental abilities made up of problems that increase in complexity in comparable units, the units in the PSI apparatus being bits. It differs from the classical intelligence tests like the Stanford-Binet and Wechsler-Bellevue in which the tasks for various age levels or subtests are entirely different in character, and consequently do not have known commensurability of dimensions. The problems of the PSI apparatus increase in complexity from a one-bit problem (push a certain button and the central light goes on)—the "earthworm floor"—to a very complicated sequence of button-pushes—the "Einstein ceiling?"—by simply adding more bits of comparable character, representing one of the three logical relationships.

By analyzing the nature of the experiments performed during the problem-solving process, taking into account their order, it is possible to relate various aspects of the process to the state of information achieved by the process up to that point, and also to relate the product of the process, the solution, to the information state. The raw data permit the quantification of a large number of variables, some of which seem to be of reasonable statistical independence. Some of these variables are *power* variables such as the time required for solution, or the number of experiments required for solution. Others are more validly *process* variables, and have consequently been of greater interest to us.

The subjects used in the part of our research to be discussed here for illustrative purposes were 59 volunteer students and staff members at the University of Chicago. This sample included a group of first-year students in the College of the University of Chicago, and Ph.D. candidates or recent recipients of the Ph.D. degree in various departments of the University. Each subject was presented with two problems containing the same total information but involving somewhat different logical structures. To our surprise, the apparently slight structural difference between the two problems resulted in a rather large functional

difference in subjective difficulty, apparently owing to the presence of a "logical loop" in one problem. Yet, even though the subjects felt that the two problems were not equally difficult, the correlations between an individual's performance on the two ranged from about .40 to .84 for our set of variables. A subset of variables correlated very low from problem to problem, about .10. It was felt that these variables were probably maximally affected by the reorganization process which ensued when greater difficulty was encountered in the solution. Only a few of these variables will be here discussed.

The variables can be grouped into three major areas of performance: methods of work, efficiency of acquisition and handling of information, and consistency and appropriateness of approach to the problem. For illustrative purposes we will present one variable from each area. A fuller presentation of the method and results is made elsewhere (4).

EXAMPLES OF VARIABLES MEASURED

Rate of work

We define the group of interrelated manipulations which occur from the time that any element on the display panel changes from zero state (becomes lit) until the first time that all elements revert to zero state (all lights are out and all consequences of activated antecedents have been displayed) as a *question*.

In a sense one might consider a question as a logical unit. Questions characteristically evolve, during the process of solution of a particular problem, from simple short series involving the direct activation of very few elements, to longer, more complex series involving the direct activation of multiple

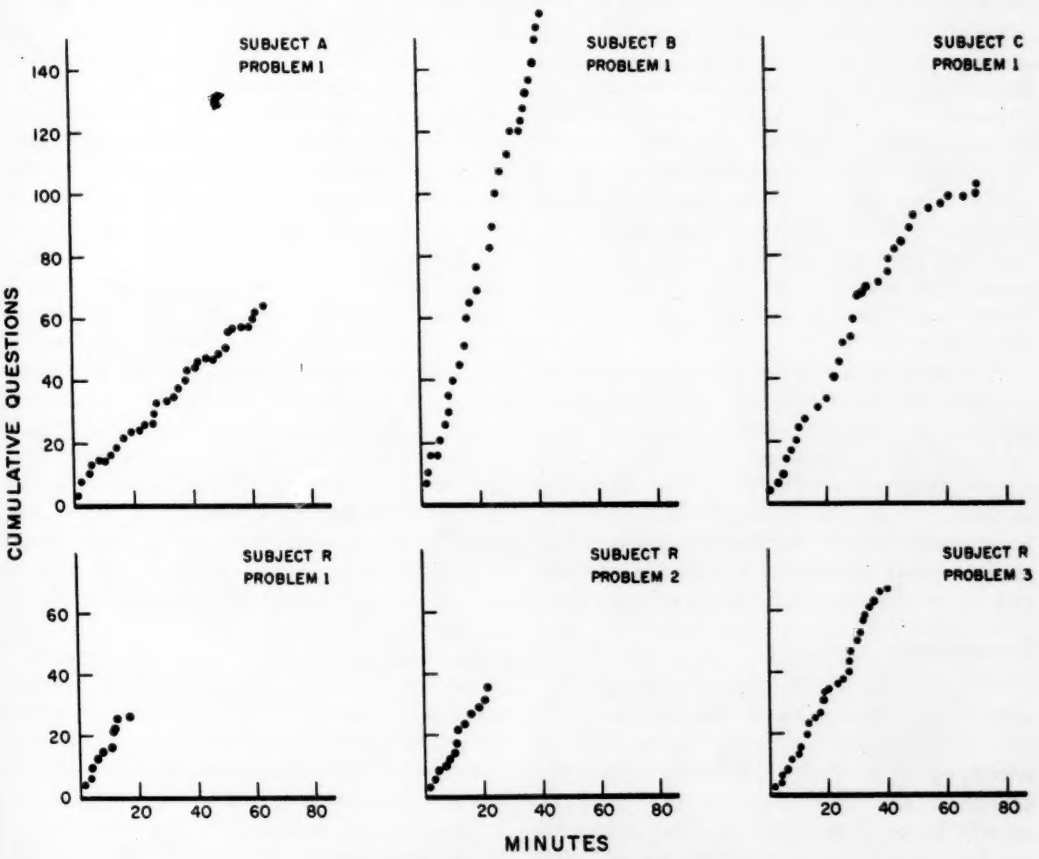

FIG. 3. Some Typical PSI Output Graphs.

elements. Presumably, after each question the subject evaluates the information gained from the phenomena observed on the display panel, and reaches a decision about how he should proceed.

A striking indication of the regularity of the processes involved in solution of the kinds of problems represented by our apparatus is obtained if one constructs an *output graph* of the cumulative questions asked during a problem versus the time at which the questions were asked. Such a graph is characteristically a good approximation to a straight line. For our sample, the correlation between the slopes of this line from problem to problem is .84. The variation among individuals on a given problem is a factor of 40. Thus it appears that individuals display different but highly characteristic *rates of reaching decisions* based on the evaluation of a body of data. We have devised a number of measures for the amount and kind of deviation from straightness of the line on the output graph, and have related these to various parameters. Frequently one observes points of change of slope in the graph, so that two straight line segments compose the curve. Such points relate highly to the achievement of certain essential information items, and we interpret them as *change in set*, by which we mean that the individual has changed the set of hypotheses according to which he was proceeding.

In Figure 3 are presented a number of output graphs. Note the consistency of Subject R from problem to problem, and the variations between the individual's slopes on the same problem. The performance of Subject B represents an unusually linear performance. Subject A represents a fairly typical amount of linearity and Subject C an unusual nonlinearity of output.

Redundancy

A number of different measures of redundancy have been developed, such as the repetition of particular questions, or *stereotypy*, the ratio of different questions asked to the total number of questions asked, the ratio of total questions asked to the minimum number which would give adequate information for solution, etc. We will here present the redundancy measure which we have found to be most reliable, and which also appears most precise. This redundancy measure represents, in a sense, the *efficiency of utilization* of available information.

From the phenomena observed on the display panel after elements have been activated in a particular question, two kinds of information are available: first, the information contained in the *overt* events themselves, what is seen, the lights that are on; second, the inferences that can be drawn from these events about other events not observed in this particular set of phenomena. Let us call the first kind of information *explicit* and the second kind *implicit*. The explicit information content of a question is invariant. A subject sees the same lights no matter when in the sequence he asks the question. (The apparatus is built so that problems can be presented in which the phenomena elicited by a question will depend on the previous questions asked, i.e., on the information state, but we are not here concerned with this type of problem, although it is most interesting.) However, the implicit information content of a question is *not* invariant. The valid inferences which can be drawn from a set of phenomena depend on what is already known. Since a subject may choose to enter the set of possible questions at many different points, there are a vast number of possible sequences of questions. In order to be able to evaluate the possible inferences which may be drawn from a particular question, it is necessary to consider the unique sequence in which this question occurs. We have constructed flow sheets for each problem, which permit the precise analysis of the inferrable information that can be derived at any point in any arbitrary sequence of questions. This has been possible because, in spite of the very large number of possible sequences of questions, there is a not too large number of interactions of questions as a function of order.

To arrive at a measure of the efficiency of use of information in a process composed of a sequence of questions, we proceed, with the assistance of the flow sheet, through the particular sequence we are studying, noting

the explicit and implicit information gained in the sequence cumulatively. We define those questions of which the explicit information yield is already contained in the total explicit plus implicit yield of the sequence of questions occurring earlier in the process as *inferrable questions*. We now define the *actual redundancy* as the ratio of inferrable questions in the sequence to the total number of questions in the sequence.

This index has proved to be quite reliable (correlation between successive performances = .60) and relates well to real-life performances requiring the analysis of underlying principles from a body of data or the application of principles to synthesize a product. (Such comparative measures were made available by subtests of University of Chicago examinations covering courses in various subject matters.)

Two factors contribute to redundancy: the total number of questions asked and the ordering of those questions. Computations show that about one-half of the variance in redundancy is due to ordering. A performance requiring almost twice as many questions as another may nonetheless be more efficient from the standpoint of acquiring information. This finding raises questions about the validity of conclusions about the problem-solving ability that are based on power indices such as time or number of questions required for solution. In psychology and education numerous judgments of ability are based on test indices of this sort.

One can define an index of the efficiency of a particular performance as 1.00 minus redundancy. In other words, a question is "efficient" to the extent that it provides new information. Although the problems used in this study required the simultaneous reconciliation of only three logical constraints and contained a relatively small number of items of information, this sample showed a uniformly low index of efficiency. By far the most effective group were the Ph.D.-level natural scientists, who had average efficiency of .35 on the two problems. That is, 65 per cent of the questions asked by this group elicited no information not already available. Least effective performance was displayed by a group of first-year college students with an average efficiency of 13 per cent. It was of interest to observe that all subgroups in the study showed approximately the same decrement (8%) in efficiency as they proceeded from the less difficult to the more difficult problem.

Analytic-synthetic shift

We define an analytic question, whose purpose we assume to be the elicitation of information, as a short, direct sequence of manipulations of the apparatus. We define a synthetic question, whose purpose we assume to be the testing of a hypothesis involving the application of previously acquired information, as a longer and more interrelated sequence of manipulations. Obviously, such a dichotomy is at best an over-simplified approximation, and where one puts the dividing line between short and simple versus longer and more complex is fairly arbitrary. After a certain amount of inspection of the actual content and context of questions in sample processes, we concluded that a reasonable criterion would be to call an analytic question one which involved no more than two successive manipulations of the apparatus, and a synthetic question one which involved more than two successive manipulations. This criterion, subject to a number of qualifying contingency statements, which need not be discussed here, was adopted and is the basis for the following discussion. If one proceeds to categorize each question in a process as either analytic or synthetic, one can make a number of statements about the process as a whole, such as the ratio of analytic versus synthetic questions which compose it. If one considers the distribution of analytic and synthetic questions throughout a particular process, it is evident that at any point in the process, the percentage of the total analytic (A) questions which occur before that point minus the percentage of the total synthetic (S) questions which appear before that point must equal the percentage of S after the point minus the percentage of A after that point. In other words, $(A - S)_{\text{before}} = (S - A)_{\text{after}}$. This difference we have called the *mixture of modes*.

We now define the point in the perform-

ance at which the difference is *maximal*, the *analytic-synthetic shift point*. This is the point at which the analytic and synthetic phases of the process are maximally separated. On one side the process is primarily an analytic process. On the other side it is primarily a synthetic process. While under certain configurations of *A* and *S* this point is not unique, such configurations are unusual. In the overwhelming majority of cases the *A-S* shift point is uniquely defined.

Using these concepts, one can investigate such questions as the relationship of analytic-synthetic shift to the state of information, the extent to which analytic and synthetic phases are distinct components of the problem-solving process, the locus of the shift point in the performance span, the frequency of reversion to analysis from synthesis and the factors which contribute to this, etc.

It is possible to stipulate the point in the process at which the necessary and sufficient information that would permit a valid solution of the problem has been achieved explicitly (NASE). It is also possible to stipulate the point at which the necessary and sufficient information to permit a valid solution has become available *implicitly* (NASI). Location of these points permits the definition of a number of intervals which we treat as variables. For example, the interval between NASI and NASE we term *inferential lag*. In this interval, the subject generally proceeds to test inferences which can be made at the NASI point, rather than to attempt to use them. The intervals between the NASI and the *A-S* shift points and the interval between the NASE and the *A-S* shift points, we term respectively the *implicit and explicit synthetic lag*. These intervals represent activity spent in excess analytic activity before the shift to synthesis occurs. If we wish to compare the performance of a given individual from problem to problem, we state these intervals as relative to the over-all performance itself. If we wish to compare individuals we use these intervals measured in absolute units such as time or number of questions.

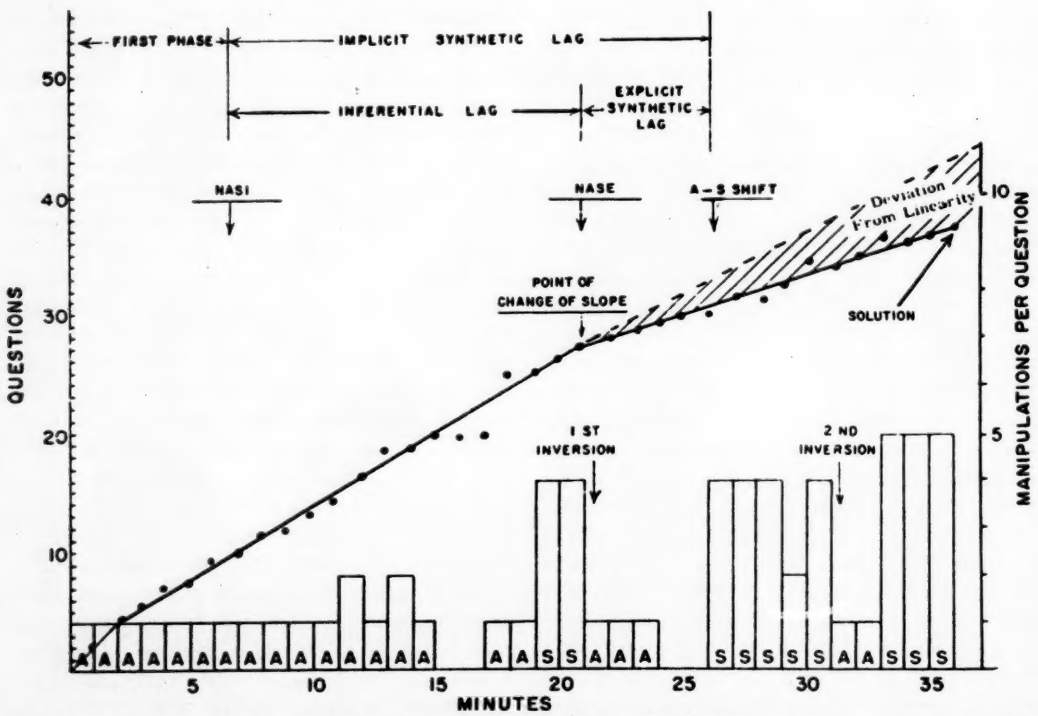

FIG. 4. Combined Output and Analysis-Synthesis Graph for a Typical PSI Performance, Illustrating the Salient Features of the Analysis of the Problem-Solving Process.

The variables mentioned above, which are based on the resolution of analytic and synthetic components of the process and the definition of the *A-S* shift, show marked differences in the correlations obtained between two problems, which range from almost .00 to .70 Most of the extremely low correlations mentioned earlier are found in this set of variables.

Figure 4 represents a typical performance, with some of the variables we have discussed above represented appropriately. It is apparent from an examination of the figure that we can begin to define and measure a number of aspects of the problem-solving process itself. We can relate these aspects to each other and to other factors such as education or performance in one or another area. Perhaps most interesting, we are in a position to quantify the effects of various experiences or training methods on aspects of the process.

SOME RESULTS ON THE PSI APPARATUS

Since our purpose is primarily to describe a technique and illustrate its utility and potentiality rather than to present comprehensively the results we have achieved, we will confine our discussion to a few of the salient aspects of our findings. Detailed results are in another publication (4).

Perhaps the most striking and surprising result is that a university group performs well below optimal levels on tasks of the sort we are here concerned with. Some members of our population actually failed to solve one or another of the problems (one subject used 469 questions in failing to solve a problem containing only 17 possible questions), although grade school children (6-10 years old) have performed adequately on the same problems on many occasions.

Correlation of certain of the variables with other available measures show that personality factors such as anxiety, perceptual factors such as speed and flexibility of closure (Thurstone's Primary Mental Abilities), and cognitive factors, are all involved in problem-solving performance. Correlations with grades in various types of course work, part scores from standardized university examinations, and other measures indicate that performance on some of the variables relates highly to the ability to perform well in tasks requiring analytic and synthetic skills.

Significant differences between first-year college students and Ph.D.-level students were absent in most measures. Further analysis showed a consistent pattern of difference between Ph.D.-level individuals trained in the natural sciences and those trained in the social sciences or humanities. These differences were such as to generate less redundant performances by those trained in the natural sciences, although those trained in the social sciences on the average reached solutions faster.

The college student subgroup was asked to write essays about themselves and their plans. On the basis of intentions announced in these brief summaries the group was divided into prospective natural science students and students intending to enter other areas of study. Comparison of differences between these two groups showed the same configuration of difference as observed between the groups at the Ph.D. level. Our failure to find significant differences between first-year and Ph.D.-level individuals was due to the presence of two subgroups in our sample at each level. Our conclusion is that the qualitative differences in problem-solving performance observed between persons in the natural sciences and in other areas is not due to factors relating to the specific training received during the Ph.D. education, but rather represents the operation of factors already present before college courses have been encountered. Developmental research might answer the question of when these two subgroups emerge in the population and what factors contribute to their appearance.

Comparison of Ph.D.-level individuals in a given specialty with college-level individuals intending to enter that specialty can be made, once one has identified the existence of these groups. Such comparison permits an evaluation of the qualitative and quantitative changes brought about in a population by Ph.D. training. In general, the effects are quantitative rather than qualitative. Natural scientists after Ph.D. training look much like prospective natural scientists before training, and the same holds for those trained in other areas.

Preliminary results indicate that it is possible, however, by structuring the situation in such a way as to impose the probability that subjects will use certain kinds of mediation processes appropriate for such tasks (5), to achieve a radical qualitative change in the performance of individuals, even though these changes do not occur if the situation is not so structured. Systematic investigation of such findings might well result in our learning to teach students in such a way as to provide them with methods of approach on which they will rely habitually in any kind of problem-solving situation, and which will be more appropriate for effective problem-solving behavior than the methods they previously used.

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If the scheme of philosophy which we now rear on the scientific advances of Einstein, Bohr, Rutherford and others is doomed to fall in the next thirty years, it is not to be laid to their charge that we have gone astray. Like the systems of Euclid, of Ptolemy, of Newton, which have served their turn, so the systems of Einstein and Heisenberg may give way to some fuller realisation of the world. But in each revolution of scientific thought new words are set to the old music, and that which has gone before is not destroyed but refocussed. Amid all our faulty attempts at expression the kernel of scientific truth steadily grows; and of this truth it may be said—The more it changes, the more it remains the same thing.

—A. S. EDDINGTON in *The Nature of the Physical World*

AGE AND THE NOBEL PRIZE

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Mozart was composing minuets when he was four years old and sonatas when he was seven. At eighteen Gauss invented the method of "least squares." Alexander the Great's famous lament came at twenty-one, and Keats had written most of his sonnets before he was twenty-four. Most men do their memorable work in their thirty years' prime of life, but Titian did some of his best painting during his seventies, and at eighty-two Winston Churchill is still writing his *History of the English-Speaking Peoples* and throwing darts at charity fairs. And Abraham was an hundred years old when his son Isaac was born unto him. Winners of the Nobel prize have also made their contributions at different ages, with those in physics and chemistry being significantly younger than those in physiology and medicine.

THIS paper reports on a study of the ages at which Nobel prize winners in the sciences did their award-winning work. The scientists studied thus worked in the fields of chemistry, physics, or physiology and medicine (hereinafter "medicine").

The investigation grew out of an interest in the question: At what age may one expect that scientific personnel will make substantial contributions to their discipline? And further: Are there any differences in this respect between different fields of science? In attempting to provide suggestions of answers to such questions one encounters the "theoretical" problem of constructing an operational definition for the concept "substantial contribution," and also the "practical" problem of either studying all persons who have made such a contribution or finding a rationale on which to draw a sample from the various classes of such people. Any number of definitions of substantial contribution may be worked out, but it was decided, for the present purposes, to designate as substantial a contribution which had been recognized by the award of a Nobel prize; we anticipate that this delineation of the group to be studied will be more acceptable to scientists than most any other selection. Simultaneously, this choice automatically provides a group of persons of recent vintage which can be statistically manipulated in several meaningful ways, and except for a very few cases there will be but little quarrel with the substance or worth of these contributions.

A few words should be said about the Nobel prize. It was distributed for the first time in 1901 and since then every year with but few interruptions; on some occasions the prize in any given field has been divided between two or three persons in that field. The decisions on awards are made by independent committees of leading scientists who have available to them the names and records of candidates proposed by especially appointed or selected institutes and persons.[1]

METHOD

A total of 164 scientists, comprising all Nobel prize winners in the period 1901–1950, was obtained. These scientists were distributed with 54 in physics, 51 in chemistry, and 59 in medicine. Comparisons of age are made between scientists in the different fields and also, for each field separately between each of the five decades (1901–1950) during which the Nobel prize has been awarded and, similarly, between the

[1] For details of selection processes, see Ref. 7.

first and second half of this period. Further observations are made on the age of the scientists at the time of receiving the award and on the time lag between performance of work and recognition by award.

The age at which a person made his contribution was obtained by subtracting the year of his birth from the year determined for his work. For the purposes of this article the latter date was defined as the date of publication of the contribution specified in the award citation; in the case of inventions patent dates were also used. In some instances the Nobel award citation refers to contributions made over a period of years; in these cases the midpoint year of the span of publication dates of central or key papers has been taken as the date of the work, e.g., scientist born in 1862, publication dates 1890–98, age at time of performing work 32.

It must be noted that the ages are approximate, probably in the majority of cases a little too high. This is due to the fact that the dates determined for the work are based on publication, and some time may have elapsed between performance of work and report by publication.

In order to ascertain whether any bias might have been introduced into the results for the different fields consequent to some uncertainty in fixing the date of work, the mean span of publication years per award-winning discovery or invention was computed for each field. The mean number of years per discovery is for medicine 4.0, for physics 3.5, and for chemistry 5.2. There are no statistically significant differences between these figures.

Sources for publication dates are too numerous to cite but consist mainly of books on Nobel prize winners (2, 3, 10), books by Nobel prize winners, selected bibliographies, and scientific indexes.

In the statistical treatment of data conventional small sample methods were used for computing standard errors of means and the significance of difference of means by the t test. Differences were considered significant at or below the 5 per cent probability of identity level.

RESULTS

The findings of this study reveal some clear differences between the scientific disciplines covered by the Nobel prize. When one considers the years 1901–1950 it is seen (Table 1) that at the time of doing the prize-winning work both physicists and chemists were significantly younger than medical scientists ($P < .001$; $P < .02$). The mean age of chemists lay intermediate between physicists and medical scientists, although the difference in age between chemists and physicists was not quite significant ($P = .06$).

Closer analysis of the data reveals that marked differences in age in the three fields were not apparent during the first 25 years of the prize from 1901–1925, but appeared in the later period 1926–1950. Thus, for 1901–1925 the mean ages were:

TABLE 1

MEAN AND MEDIAN AGES OF NOBEL SCIENTISTS AT TIME OF DOING AWARDED WORK

Period	Medicine		Physics		Chemistry	
	Mean	Median	Mean	Median	Mean	Median
1901–10	39.4 ± 0.3	39.5 (12)	38.0 ± 2.5	38.0 (14)	35.2 ± 1.3	35.0 (10)
1911–20	38.8 ± 3.5	35.0 (6)	36.5 ± 2.6	34.5 (10)	34.8 ± 2.6	33.5 (8)
1921–30	40.6 ± 2.3	39.0 (11)	33.8 ± 2.2	32.5 (12)	42.3 ± 1.7	42.5 (10)
1931–40	43.9 ± 1.9	43.0 (13)	33.3 ± 2.2	31.5 (10)	38.1 ± 1.4	37.5 (12)
1941–50	44.2 ± 1.8	43.0 (17)	34.8 ± 2.8	35.5 (8)	40.0 ± 3.6	37.0 (11)
1901–25	39.1 ± 1.2	38.0 (23)	36.7 ± 1.5	34.5 (30)	35.6 ± 1.2	35.0 (22)
1926–50	43.8 ± 1.2	43.0 (36)	33.9 ± 1.3	32.0 (24)	40.3 ± 1.4	39.0 (29)
1901–50	41.9 ± 0.9	41.0 (59)	35.4 ± 1.0	33.0 (54)	38.3 ± 1.1	37.0 (51)

The number in each group is given by the number in parentheses.
Standard errors are indicated.

chemists 35.6, physicists 36.7, and medical scientists 39.1. Only the relatively small difference between chemists and medical scientists is significant ($P = .05$). On the other hand, for the 1926–1950 period the corresponding figures are: chemists 40.3, physicists 33.9, and medical scientists 43.8. The mean age difference ($P < .001$) of almost 10 years between physicists and medical scientists is most striking. The age difference of chemists and physicists is also significant ($P < .01$), but that between chemists and medical scientists falls short of significance ($P = .06$).

Almost as remarkable as the interdisciplinary differences are the intradisciplinary changes in the age at which scientists did their prize-winning work during the earlier and later period of the Nobel prize. There is a clearly significant increase in the mean age of awarded scientists in both chemistry ($P < .02$) and in medical science ($P = .02$). The apparent decrease in the age of physicists is not statistically significant. Inspection of Table 1 shows the tendency toward increase in age in chemistry and medicine when the figures are further broken down into decades, but the size of N is too small for these periods to permit of any statements.

There is one feature of the 1901–1925 period of the prize which might tend to make the aforementioned differences appear spurious. It should be remembered that in the first ten years of the Nobel prize there was a backlog of candidates who did their work before the institution of the award. This factor of backlog cannot account for differences among the fields since the proportion of persons receiving the prize during the first 25 years, who did their work before

TABLE 2

PERCENTAGE OF SCIENTISTS IN DIFFERENT AGE GROUPS AT TIME OF DOING AWARDED WORK (1901–1950)

Field	Age Groups									N
	21–25	26–30	31–35	36–40	41–45	46–50	51–55	56–60	61–65	
Medicine	0.0	1.7	13.5	33.9	23.8	15.2	8.5	1.7	1.7	59
Physics	9.2	20.4	27.8	16.7	13.0	9.2	3.7	0.0	0.0	54
Chemistry	2.0	7.8	31.4	25.5	19.6	5.9	5.0	2.0	0.0	51

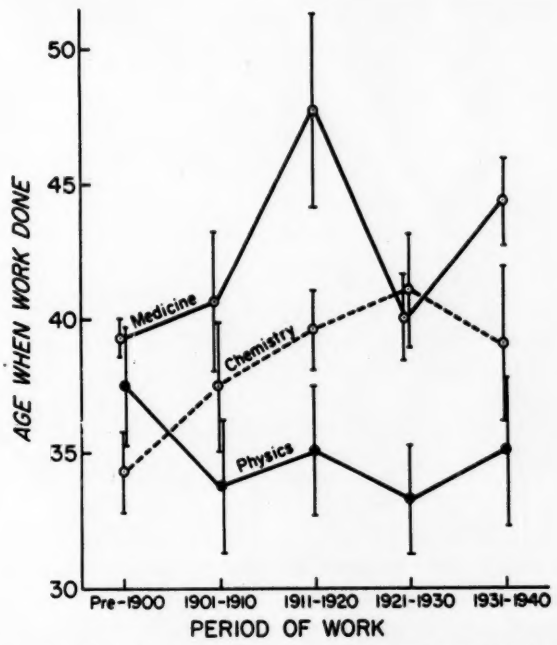

FIG. 1. Age at the time of publication of prize-winning work plotted against date of publication. Means for the publication period 1941–1950 have not been included in this figure as only one or two individuals in each field are involved. Standard errors of means are indicated by vertical lines.

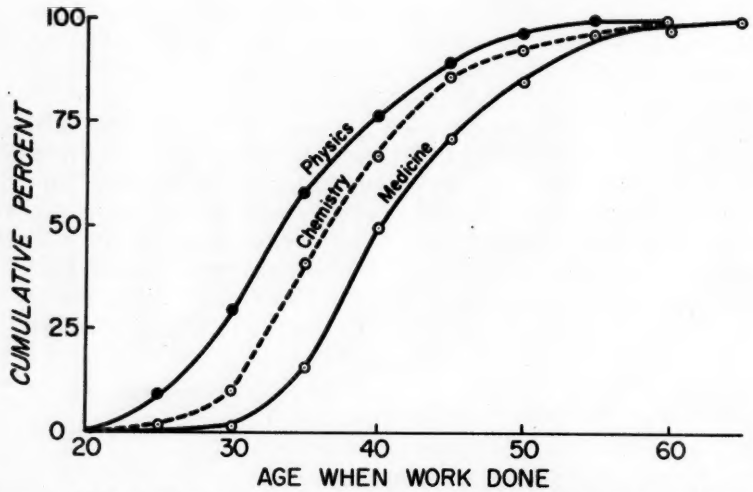

FIG. 2. Percent of Nobel prize winners in each field who published their contribution at or before a given age.

TABLE 3

MEAN AND MEDIAN AGES OF NOBEL SCIENTISTS AT TIME OF RECEIVING NOBEL AWARD

	Age (Years)					
	Medicine		Physics		Chemistry	
Period	Mean	Median	Mean	Median	Mean	Median
1901–10	56.0 ± 2.0	56.0	50.9 ± 3.0	50.5	52.2 ± 3.0	51.0
1911–20	47.7 ± 3.7	47.5	46.7 ± 3.7	46.0	48.1 ± 2.2	46.5
1921–30	54.1 ± 4.4	62.0	44.2 ± 2.4	42.0	51.6 ± 2.2	51.0
1931–40	54.0 ± 3.8	49.0	38.8 ± 4.0	41.0	45.8 ± 2.2	47.5
1941–50	56.2 ± 2.3	54.0	50.6 ± 2.5	49.0	55.6 ± 2.8	55.0
1901–25	51.1 ± 1.2	49.0	47.8 ± 1.5	46.0	50.4 ± 1.2	49.5
1925–50	56.9 ± 1.2	56.0	44.6 ± 1.3	45.5	51.1 ± 1.4	51.0
1901–50	54.6 ± .9	54.0	46.4 ± 1.0	45.5	50.9 ± 1.1	50.0

Standard errors of means are indicated.

the turn of the century, is nearly identical for all of the fields with 56% in medicine, 59% in physics, and 57% in chemistry. Moreover, the apparent increase in age in more recent times among laureates in chemistry and medicine, as well as the divergence in age of the former from that of laureates in physics, is borne out when considered not only with reference to the time base of years of Nobel award, but also on the time base of the period in which they worked. This is illustrated in Figure 1.

Obviously, this alternate way of looking at the data gives another time perspective and has the additional advantage of minimizing the variable of time lag between performance of work and award of the prize.

Age differences between the fields are further illustrated in Figure 2, which is a plot of the cumulative percentage of all prize winners in each field who had performed their prize-winning work at or before a given age; this figure shows a feature not previously mentioned in the discussion of mean age, i.e., the extremes of the age scale. Table 2 shows percentage of scientists in the three fields distributed in categories of 5-year age groups; here two interesting differences between the fields are manifested.

By age 30, only one Nobel laureate in medicine (1.7%), F. G. Banting, had done his prize work; yet by this age 30% of the physicists and 10% of the chemists had published their prize work. Both W. L. Bragg in physics and T. W. Richards in chemistry did their prize work at age 23. In fact, 10% of all laureates in physics have done their award-winning work by age 25. At the median age for physicists, 33 years, only 10% of the medical scientists and 25% of the chemists have done their award-winning work. At the other end of the scale, 12% of medical scientists did their work after age 50 while the corresponding figures are 7% for chemists and only 4% for physicists.

If one examines the age of Nobel laureates at the time of award of the prize (Table 3), it is found that in general the same statements regarding field differences and tendencies apply. Thus no significant difference between the fields was found for the period 1901–1925. The mean age for all prize winners was 49.5 years. On the other hand, during the period 1926–1950 marked differences between the fields are evident with the largest difference seen between the ages of laureates in physics and medicine (respective means are 44.6 and 56.9 years). As noted previously, the mean age in chemistry is intermediate and significantly different from both physics and medicine. The mean age in physics is significantly lower than the mean age in medicine ($P < .001$) and also significantly lower than the mean age in chemistry ($P = .03$). The mean age in chemistry is significantly lower than the mean age in medicine ($P = .02$). The mean age of medical scientists during the period 1926–1950 had significantly increased over the preceding 25-year period ($P < .05$), whereas the age at award in physics and chemistry did not change significantly when the two 25-year periods were compared.

It is of interest also to examine the distribution of age at the time of award of the Nobel prize (Table 4) over the 50-year period. At the median age for physicists, only 20% of medical scientists and 28% of chemists had been named laureates. Whereas 7 physicists (13%) had won the prize by age 35, only one (1.7%) in medicine and one (2%) in chemistry had done so. At the other end of the distribution, 10 laureates (17%) in medicine were awarded the prize after age 65, while the corresponding figures for chemistry and physics are two (4%) and one (1.8%). It is worth noting that the distribution for age at award in medicine is bimodal with the most frequent age groups being 46–50 and 61–65[2].

TABLE 4

PERCENTAGE OF SCIENTISTS IN DIFFERENT AGE GROUPS AT TIME OF AWARD (1901–1950)

Field	Age Groups											N
	21–25	26–30	31–35	36–40	41–45	46–50	51–55	56–60	61–65	66–70	71–75	
Medicine	0.0	0.0	1.7	8.5	10.2	20.4	11.8	10.2	20.4	11.8	5.1	59
Physics	1.8	0.0	11.1	16.6	20.4	13.0	14.8	14.8	5.6	0.0	1.8	54
Chemistry	0.0	0.0	2.0	7.8	17.6	25.5	19.6	15.7	7.8	2.0	2.0	51

TABLE 5

MEAN INTERVAL BETWEEN WORK AND AWARD, IN YEARS

	Medicine	Physics	Chemistry
1901–1925	12.0 (1,27)	11.1 (2,37)	14.8 (3,35)
1926–1950	13.1 (2,32)	10.7 (2,27)	10.8 (1,24)
1901–1950	12.7 (1,32)	11.0 (2,37)	12.6 (1,35)

Numbers in parentheses are shortest and longest interval between work and award.

How many years elapsed between the performance of scientific work and its recognition as a substantial contribution? Data relevant to this question are provided in Table 5. Considered over the first 50-year span of the prize, there is a mean interval of 12 years between the time of work and the time of award. Although each field has its own selection and award committee, there are no significant differences between the fields with respect to the time lag between work and award. The range is from vir-

[2] The bimodal distribution in age at award in medicine does not interfere with the general statistical treatment in this paper as it is the only bimodal distribution encountered; the weakness of a statistical argument drawn from this particular distribution is recognized.

tually immediate recognition to a lag of 37 years. The large range is evident in all fields. We note that the mean interval for any one field has remained relatively stable with no significant differences for the period 1901–1925 as compared with the period 1926–1950.

DISCUSSION

A considerable body of data has been developed and summarized by Lehman on relationships between age and productivity in various fields of activity (6), and a good many older works also treat on the same topic. Some recent articles (1) indicate current interest, but none of these contributions are of such a character that any direct comparisons can be drawn between them and the present study. This holds for both findings and analyses.

This study, as others in this area, has been unable to develop a sample of scientists for study. Nevertheless an interpretation of the findings might offer, at any rate, suggestions of differential conditions in the groups or fields at issue. Any such interpretation would have to account for both differences between the fields and for differences within one field at different points in time. The data available are clearly wholly inadequate for such an analysis, yet several factors come readily to mind as possible explanations for parts of the observations. One hypothesis would find the significant elements in differences in the personality (including intelligence and work habits) of the persons attracted to and recruited into the various fields at different times. Several studies have addressed themselves, directly or peripherally, to this problem; articles by Kubie (5) and Roe (8, 9) represent one approach in this endeavor. A somewhat different orientation, probably of greater potential in the present connection, may be seen in the discussion by Holton (4) of the interaction between what he calls "private" and "public" science. Although not yielding any direct contributions to the data in this article, these studies may be of suggestive value in the framing of specific hypotheses.

Alternate or additional hypotheses would investigate possible different time requirements for professional training in the there academic fields, concurrent with an examination of the relative adequacy of the skills and knowledge provided by such training. It is also conceivable that the social organization of the fields vary with respect to the opportunities and support afforded scientists at different stages of their career, but perhaps especially in the initial stage, relative to long-range research of independent, significant, and perhaps deviant character. In connection with research training it may be remarked that in the first 25-year period the large majority of recipients of the award were Europeans and very few Americans figure in the roster. During the second 25-year period the proportion of American recipients has grown considerably.

It might be thought that the greater age of medical scientists, especially, has followed from a greater demand of time required in medical research, i.e., medical research projects are slower in developing conclusions due to some characteristic of the medical project or the nature of its material. But we do not wish to imply that Nobel prize winners in medicine worked for a longer period of time on their contribution than did laureates in other fields. Although it is quite obvious that the older scientist would have worked in his field for a longer period of time, it does not follow that he has worked any longer than his younger colleague with a particular approach or on a particular problem before bringing it to a meritorious result. We have not specifically tested this point, and a simple review of literature and publications clearly cannot suffice since scientific invention or discovery[3] depends on, at any rate, a complex (i.e., unknown) interaction between the stage of development of the field, its supporting body of knowledge and conceptual framework, and the activities of the individual scientists engaged in "private science." The data do indicate, however, that with respect to "public science" (public communication of procedures and observations, etc.) the mean span of publi-

[3] In order to permit easier communication we have used the term "discovery," although it would be more in keeping with current views on the methodology of science to discard this term in favor of "invention" as conceptual or symbolic invention.

cation dates of key articles, which constitute the basis for award, does not differ among the fields despite the mean age differences among the scientists in the three disciplines. Thus the least that can be said is that so far we have been unable to find support for the view that the higher age of medical scientists is a function of greater time requirements in medical research than in physical or chemical research.

Perhaps the major part of the explanation of the observed distributions may be found in the organization and development of the three disciplines at hand. As one compares the work selected for award in physics with that selected in medicine, one is struck by the contrast between the two. The majority of the work in physics involves systematization, or discoveries which were predicted by such systematizations or by generalizations arising from the changing notions of the nature of the physical (observable) world. On the other hand, the large majority of the work in medicine consists of relatively discrete observations, e.g., investigations of particular functions of an organ system or isolation or synthesis of compounds (as vitamins, hormones, chemotherapeutic agents, etc.). While one thinks of medicine as becoming more quantitative (beyond the mere use of "statistics"), mathematical, and general, this tendency has thus far not been widely reflected in the works selected for the Nobel prize.

The list of awards in chemistry during the first 25 years of the Nobel prize includes a high proportion of contributions closely allied to physics. This was the period of the rise of physical chemistry and mathematical formulation. In addition, there were many works associated with the research on atomic structure. With the development of biochemistry, there is a large representation of this discipline in the list of awards in chemistry roughly in the period 1926–1950.

What is suggested by the preceding is that scientists working in a field in which significant contributions are related to consequent systematization of predictive knowledge may make their discoveries at an earlier age than those working in a discipline in which detailed phenomenological knowledge constitute the main "body" of that discipline. Of the three disciplines involved, physics underwent the most marked reorientation around the turn of the century (Röntgen, Planck, Einstein, etc.) which opened up the way for a number of new and major conceptualizations and discoveries (including new ways of looking at existing observations). No comparable situation has obtained in medicine, mainly physiology, during the current century (i.e., since the institution of our criterion for a "substantial contribution"). If either physiology or chemistry should experience a basic rearranging of its material, it should presumably be possible for relatively young and inexperienced persons to make major contributions on the basis of a new conceptual scheme in an open field. Whether this particular evaluation of the three fields vis-à-vis each other is accepted or not, it still seems plausible that the relative development and condition of the fields will have to be taken into account.

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COMMENT

Language as Behavior

On Human Communication; A Review, A Survey, and A Criticism. Colin Cherry. London: Chapman and Hall, Ltd; New York: John Wiley & Sons; Cambridge, Massachusetts: The Technology Press of Massachusetts Institute of Technology, 1957.

Language as Choice and Chance. G. Herdan. Groningen (The Netherlands): P. Nourdhoff N. V., 1956.

In mapping the territories of behavioral science one finds areas more thoroughly and less thoroughly explored and also areas which, one feels, are more or less amenable to exact or mathematical treatment. In particular, psychophysics and learning theory have proved themselves "mathematicizable," and some aspects of the language sciences predominantly so. Elsewhere in this journal[1] a role was forecast for "mathematical linguistics" analogous to the role of mathematical genetics in biology. That this idea is shared by at least some students of language behavior is apparent in the two books before us.

One of them, *On Human Communication* by Cherry covers as wide a field as its name implies. Indeed a statement on the jacket describes the book as the "first single volume to appraise the relationships existing between the diverse studies of communication." One could take issue with this claim recalling George Miller's *Language and Communication* (1951), which introduced the nonspecialist to the phonetic approach to language, the statistical approach, the problem of determining speech perception, semantics, the relation of linguistics to learning theory, the role of verbal habits in behavior, and the social aspects of communication, a selection of topics roughly equivalent to those covered by Cherry. However, the uniqueness of the book is not an important issue, especially since, in all likelihood, more books of this kind will soon appear. The important point is the recognition of the intimate interrelation between the various aspects of language science—the physical (phonetics), "strictly" linguistic (phonemics, "morphemics," etc.), the statistical, the psychological, the technological (as in telecommunication), the semantic, and the social.

Cherry's book, as so many books with scientific content nowadays, is addressed hopefully to the nonspecialist in the sense that no topic is treated technically in depth. However, the inclusion of chapters on the mathematical theory of communication cannot but indicate that the intended reader has a deeper interest in communication than the generalized "intelligent reader." Who, then, is this nonspecialist to whom "a review, a survey and a criticism" of human communication is addressed? Can he be the "behavioral scientist," that is, someone whose interests have spilled over from any one of the established disciplines, having a bearing either on behavior directly (biology, psychology and the social sciences) or on language (linguistics, semiotic) or on method (philosophy, mathematics), into the newly recognized area of "behavioral science," in which all these disciplines are supposed to merge?

Whether this conjecture about the intended reader is correct or not, we shall assume it is (keeping in mind also the "intelligent layman") and shall evaluate this book on that basis. Indeed we find that the most favorable evaluation results if we assume that some chapters are addressed to the "intelligent layman" and others to a reader with at least some scientific training. If one writes for these two audiences, a fluctuation in the level of technicality is practically unavoidable. The reader must also understand this and accordingly must be tolerant when the discourse seems too elementary and patient when it seems too technical. Striking a "mean" between a popular exposition and an account of one

[1] Gerard, R. W., Kluckhohn, C., & Rapoport, A. Biological and cultural evolution: Some analogies and explorations. *Behav. Sci.*, 1956, 1, 6–34.

specialized field for a specialist in another field is a most difficult task. Often the only way to do it is to pass repeatedly from one level to the other, so that at least an impression of such a "mean" remains. The author recognizes this problem and says so.

Another problem, clearly recognized by the author, is the Babel of terminology. Indeed the task of unifying the "disciplines" revolves not so much around showing that various specialists have different names for the same concepts as around showing that they use the same terms for widely different concepts and, what is much worse, apply widely different standards of rigor in the definitions. Accordingly a glossary of terms is provided in which the meanings of the terms stem from usage in "communication theory." These terms range mostly over the vocabulary of communication engineering and that of technical philosophy of language (semiotic) and thus reveal by implication what the author means by "communication theory."

Finally, a word is in order concerning the context in which this book was published. It is announced as the first in a series of volumes intended to deal with studies in communication. Mr. Cherry is a Reader in Telecommunication at the Imperial College, University of London. Subsequent volumes are promised by an anthropologist, a linguist, a logician, a social psychologist, and others. Evidently this first volume is meant as an introduction rather than as the telecommunication specialist's point of view.

The first chapter, entitled "Communication and Organization—an Essay," clearly reveals the author's view of communication as encompassing the entire scope of human behavior, particularly social behavior. The "text" introducing the chapter is the Genesis story of Babel. One cannot help recalling a satirical piece of two or three years ago[2], in which the direct obverse of the story was offered as explanation for the breakdown in communication. There the builders of the Tower first spoke each in a different language. The few simple gestures asking and offering help sufficed to keep the work going. Thereupon the Lord, wishing to confuse them, *united* their tongues. In the ensuing arguments involving building methods, the purpose of the Tower, and evaluation of individuals' efforts, the work soon stopped.

At any rate the emphasis in the introductory essay is on human communication as a social component of structure. Society is an "organism" or a system kept together by its networks of organization and communication along which the integrative functions of this organism operate. Emphasis is also given to the comparatively new mathematical tools presumably appropriate to deal with the exploration and description of such networks—the mathematics of topological graphs.

One may note in passing that hardly any mathematician who has had experience with this sort of mathematics will fail to agree that if any branch of mathematics is appropriate for the study of sociograms, organizational and communicative patterns, and especially for quantifying degrees of complexity of systems, it is decidedly the theory of topological nets or "graphs." One must recognize, however, that although treatises on graph theory exist, and although topology is one of the most intensively developed branches of modern mathematics, applications of these methods to the study of communication and organization are still very rare and so far confined to a most elementary analysis. Kurt Lewin was one of the first to feel (probably rightly) that "topological psychology" is a fertile field. But there is no topology in Lewin's psychology. Topology is very advanced and very technical mathematics. Its effective use requires, besides a feeling of its importance, a virtuoso-like command of its techniques which so far is found only among topologists (i.e., not even among all mathematicians). Nor can these techniques be taught to a non-mathematician. That is to say, once one commands these techniques, one has become a mathematician. Therefore until full-statured topologists become sufficiently interested in applications of this sort, there is little hope that the theory of organization and of communication patterns will advance into this

[2] Salomon, L. B. A gospel-true fable. *ETC., A Review of General Semantics*, 1954, 12, 3–14.

richly promising territory. If we add to all this the circumstance that to deal with large aggregates, statistical methods are indispensable, we see that effective work in this area requires a pooling of topological, statistical, and social-organizational knowledge, a formidable requirement.

Chapter 2 is a historical survey of communication science. The meaning of a "code" is explained and elaborated by several lucid examples, including the phenomenally sophisticated Ogam (Celtic) binary alphabet. The mathematical theory of communication (loosely called "information theory" in the United States) is previewed (to be developed in later chapters). The chapter closes with a section on "scientific method," actually a sketch of the theory of mathematical induction.

There follows a chapter on linguistics in its broadest aspects, from the most "physical" level of phonetics through the elementary units of "pure" linguistics, the phonemes and their analysis into distinctive features, to semantics, and the esthetic components of verbal expression. This too is a preview of a later more systematic exposition of three levels of the theory of signs developed a decade ago by Charles Morris, syntactics, semantics, and pragmatics. The work of Zipf on statistical distributions in language samples and its interpretation by Mandelbrot are briefly described, again in preparation for later more detailed treatment, following the exposition of the mathematical theory of communication.

Chapters 4 and 5 contain the bulk of the technical treatment based on the author's special field of competence—telecommunication. The most significant statement in the book, in this reviewer's opinion, is contained in the following declaration of the responsibilities of the engineer:

> An engineer is concerned not only with the design of apparatus, but with its operation by human beings. The civil engineer builds bridges for people to use. The mechanical engineer designs a motor car to operate with its driver, as an integrated mechanical-human unit;[3] a unit, we notice, not a man *plus* a machine—and a unit with its own set of reaction times, its own behavior, unlike those of a normal man (incidentally, since its freedom of action is so different, we should expect this unit to have its own moral code). A telecommunication system too is designed to work together with human users, and the engineer is concerned that it should work successfully as a unit (p. 122).

Such sentiments are familiar to the readers of this journal as associated with thinking in terms of "systems." The consequences of such thinking are not only philosophical but also heuristic and practical. To quote further, ". . . although it is 200 years since the first telegraph line was set up, it is only during recent years that engineers have considered how they may measure not only the signals, their wave forms and their spectra, but *the information* which their electrical channels have the capacity to transmit."

One may add that although efficient design of telegraphic systems has long antedated a thoroughgoing analysis of the information conveyed by the signals (intuitive feelings of the designers sufficed in this case), this is not true of the more complicated telecommunication systems, telephone, radio, and television. For where the human voice is involved and its receipt by the human ear (or pictures and their receipt by the eye), the human links in the communication chain become of dominant importance. Here engineering knowledge cannot be effectively applied without being supplemented by knowledge of the physiology of the senses and possibly of psychology. Thus a link must be made between at least physics, physiology, and engineering.

A central theoretical position in this field is occupied by the mathematical theory of wave forms, and the central position in this theory is occupied by the so-called "sampling theorem" which states that a wave form transmitted in a finite time and limited to a

[3] "A car is just an extra limb in Junior's estimation; Between them there exists a dim umbilical relation; And he is it, and it is him by transsubstantiation." From a poem published in *The New Yorker* some ten years ago with apologies to the author, whose name I cannot recall.

finite bandwidth is completely determined by its values at a specified finite number of points. This means roughly that a continuously flowing signal may be *completely* transmitted if only "samples" of it are sent at the right frequency (as if, say, a complete message could always be reconstructed from every tenth "letter"). This result governs the design of modern telecommunication equipment where knowledge of the characteristics or our speech and sensory apparatus indicates the bandwidth requirements of channels. Through this discovery great impetus was given to the investigations of the physical characteristics of speech.

Chapter 5, covering roughly the fundamental work of Hartley and Shannon on the mathematical theory of communication is a welcome addition to the now numerous expositions of this subject. It is among the most lucid of these expositions. Within some 50 pages the material covered includes the definitions of basic concepts, examples of calculation of information in stationary sources, a treatment of redundancy (with elementary, well-chosen examples), coding to take advantage of redundancy to combat noise, "continuous information," the crucially important idea of statistical matching of communication systems to sources, some fundamental mathematical and statistical concepts which underlie the treatment of the subject, and a more detailed treatment of Mandelbrot's application of the mathematical theory to statistical distributions in language samples (particularly to Zipf's law). The chapter ends with a discussion of the information-entropy relation.

We have suggested that the volume is implicitly addressed to two classes of readers, the "intelligent layman" and the "behavioral scientist." The author explicitly recognizes only the first of these. The treatment of the mathematical theory of communication, however, seems most suited to the needs of the latter particularly because of its emphasis of the exact scope of the exact mathematical theory of communication. There is still a formidable amount of loose talk in various branches of behavioral science about the "break through" facilitated by "information theory" on all fronts from the biophysics of enzymes to the flow of "information" in society. The author points out, as several others have, and, one hopes, with telling effect that (*a*) the mathematical theory of communication is entirely on the "syntactical" level, where the communication process is entirely abstracted from the content of communication; (*b*) that the formal analogy between information and "negative entropy" has physical meaning only at the molecular level of statistical mechanics—it has *not* been extended to the level at which we perceive formally defined "information" as information without quotation marks. For this reason, the distinction made in Britain between the theory of communication and information theory (terms which have become almost synonymous in the United States) is emphasized. The author suggests that the term "information theory" be reserved for that branch of scientific method which deals with obtaining information from nature, a field in which Sir Ronald Fisher has been a pioneer. As has been pointed out (among others by Wiener, I believe), there are links between that information theory and the mathematical theory of communication. But the connection is not at all obvious. To quote the author once more,

> Questions of extracting information from Nature and of using this information to change our models of representations lie outside communication theory—for an observer looking down a microscope or reading instruments, is not to be equated with a listener on a telephone receiving spoken messages. Mother Nature does not communicate with us with signs or language. A *communication channel* should be distinguished from a *channel of observation*.

In spite of all the *caveats* against naive "metaphorical" extension of the concepts developed in mathematical communication theory to methods dealing with the problems of "meaningful" communication, the pressures for such extensions will continue. At any rate, the conquest of one class of communication problems by exact methods makes for an irresistible temptation to extend the conquest, especially since the area of "meaningful" communication has

been mapped in the last half century in the course of investigations, which although not mathematically exact, have been on a respectable level of logical rigor. Chapter 6 is a survey of these investigations, particularly of the systematization of communication problems into syntactics, semantics, and pragmatics.

This branch of knowledge has been traditionally rooted in epistemology, particularly in its development by the English Empiricists (especially Locke and Hume), and in the profound speculations of Kant. The revival of interest in the theory of knowledge which occurred toward the end of the last century (probably spurred on by the collapse of the Euclidean-Newtonian system) culminated in the work of the Vienna Circle, especially in the writings of Carnap who rephrased the problems of epistemology in terms of "logical syntax." Meanwhile another direction branched off, as is apparent in Lady Viola Welby's remarkable article, *Significs*, in the Eleventh Edition of the *Encyclopedia Britannica*. As many American readers may know, "significs" was the point of departure of Alfred Korzybski, who attempted to construct, on the basis of what he called a "non-Aristotelian system," a neurolinguistic theory of behavior and a psychotherapeutic discipline, "general semantics." Neither general semantics nor Korzybski is mentioned in the book, but it is notable that the chapter subheading discussing the subject is entitled "Significs—or Mental Hygiene." This identification certainly brings communication theory well within behavioral science. Whereas syntactics is concerned only with relations among signs, semantics with the relation of signs to their referents, pragmatics treats of the relations between the signs and the *users* of the signs. The subject matters of significs, pragmatics, and "general semantics" are practically identical.

Thus the chapter on semiotic is another condensation of a vast field of inquiry. Emphasis is given to the theory of *semantic* information, largely the attempts of Bar-Hillel based on Carnap's theory of inductive probability, to make a rigorous extension of information theory to meaningful propositions. An examination of the scope of that extension will convince any serious student of how little can be done with the present level of theory if one is determined to stay within the bounds of rigorous treatment.

The subjective aspect of probability ("degree of belief") is passed over quickly, one might say too quickly, in view of the increased output of recent work on that subject, but perhaps justifiably so, in view of the scope of the volume. The omission of metalinguistics, however, is less justifiable in a survey of this sort. The absence of, say, the writings of Benjamin Lee Whorf from a bibliography of 367 entries is quite conspicuous.

Finally the implications of the theory of signs for scientific method are outlined. This reviewer must take issue with one passage, in italics in the quotation below, which, in fairness to the author's intention is reproduced in the context of its paragraph.

> Scientific theory rests upon statements, upon sets of axioms. These statements present relations between various terms; to the syntactical relations between these terms is correlated, we imagine, relations between things and events in the real world. *But science is not concerned with the "meanings" of the various terms, only with their relations*. . . (p. 252).

If the author has inadvertently substituted "science" for "scientific theory" in the last sentence, then the passage is simply a reflection of what the author means by "scientific theory," namely the purely deductive aspect of scientific discourse (even "induction" belongs to this formal deductive aspect once its rules have been rigorously formalized). But to say that "science" is not interested in what its terms stand for, that is, in the realizability of its theories is to deprive science of its *raison d'être*. There are indeed branches of behavioral science where unfortunately but, perhaps, for the time being necessarily, the *only* concern is with a systematization of correspondences between terms and observables.

The last chapter, the most "psychological" one, deals with cognition, especially recognition. The central problems of Gestalt perception and of the selection of stimuli from the environment are reviewed and adroitly set into the context of communication theory.

Altogether the survey is a remarkably successful compression of a vast collection of characteristically modern ideas on the nature of the most human of all human attributes. It not only fulfills a need in this day of increased interdisciplinary efforts but can also serve as a stimulant for attracting future scientists to work in the border areas connecting the physical, the biological, and the social sciences.

In *Language as Choice and Chance* by G. Herdan, the emphasis is primarily on quantitative *empirical* studies of language behavior. Two circumstances make language especially appetizing for the statistician: the identifiable units and the volume of the output. True, the units have turned out to be not as well defined as they are made to appear in printed samples (words, letters), but discrete units *have* been established to a degree in modern linguistics, and so the statistical basis (the possibility of counting) is also established. As for the output, it remains vast by any standards and so provides fertile soil for statistical studies.

The starting point is de Saussure's conceptual distinction between *la langue* and *la parole*. The first refers to the language as the totality of "word-engrams" possessed by the users of the language; the second to the actual occurrences of language behavior. The definition can be sharpened if *la langue* is considered as that from which a dictionary (including all the variants of each word) can be compiled, while *la parole* constitutes the several selections made from that repertoire by individual speakers.

The author proposes to bring this definition into closer contact with statistics by imposing the over-all frequencies of occurrence onto *la langue*. If this is done, then *la langue* becomes a population (a statistical universe) and *la parole samples* from that universe. The first statistical problem then becomes apparent: to infer *la langue* (the population) from *la parole* (repeated samplings).

The question naturally arises with what distributions should the statistical linguist be concerned? The answers to this question are many and until some *theories* of distributions appear, especially rival theories, one answer is as good as another. Judging from the examples given, statistical linguistics is still largely in the descriptive stage.

The first two parts of the book are devoted mainly to exhibiting compilations of data on occurrences of various elements, stylistic, grammatical, orthographic, phonemic, etc. in the writings selected from various authors, languages, and periods. To mention a few, we are introduced to the relative frequencies of "resolutions" in seven plays of Sophocles; percentages of Romance words in thirty works of Chaucer, frequency distributions of words in the writings of Churchill, Halifax, Streseman, Beneš, Stalin, and Hitler, frequencies of parts of speech in Dante's *Divine Comedy* (broken down into "Inferno," "Purgatorio," and "Paradiso"), relative frequencies of various consonants in Greek, Latin, and Gothic, various comparisons between conversational and nonconversational material, occurrences of *caesurae* in ancient Greek hexameters, and very many more such items (there are over 100 tables in the book).

It seems there are two classes of questions in statistical linguistics, one concerning the statistical significance of the data, the other concerning the meaning for linguistics of the statistically significant findings. The first class of questions can be answered unambiguously in the light of statistical theory. The second class is directed toward the search for significant indices of language behavior.

The tremendous verbal output available, at least where written language occurs, nourishes the hope that regularities on the grand scale can be observed, described, and watched. Just where to look for these regularities is another question. What does one count? Frequencies of occurrence of all words? Length of words? Multiple meanings? Parts of speech? Syllables? Phonemes? All of these have been tried and many more. Apart from establishing the existence of regularities (as must exist in any mass phenomenon—the more entities are involved, the sooner must the laws of chance come into their own) not much more can be immediately abstracted from this procedure.

Any observed regularity yields an "index."

Obviously any of these indices can be combined in many ways thus yielding new derived indices. A number of these have been proposed and measured for a variety of samples and languages.

One is impressed with the tremendous amount of work which must be involved in this counting procedure. It is reasonable to hope that somehow a coherent picture will emerge which will indicate more than straight empirical answers to straight empirical questions like, "What is the ratio of the frequencies of /z/ to /p/ in English and Albanian respectively?" In the meantime one supposes that the laborious process must go on, especially since some workers appear to be devoted to it.

Here and there glimpses appear which seem to bear a promise for linguistic "diagnosis." It is maintained, for example, that the Russian prose of Turgieniev bears a closer resemblance in its statistical attributes to German than does the prose of other Russian writers. In the light of the general recognition that Turgieniev was the most "Europeanized" Russian writer of his generation, this is a highly interesting discovery. Unfortunately it is just at such moments where supporting evidence is not offered.

One might say, in passing, that if verbal output merits such laborious investigation, then certainly music does. Styles in music are clearly recognizable by even average-powered connoisseurs both with respect to individual composers and periods or national schools. It may be that statistical regularities analogous to those of language are even more pronounced in music which is not nearly so encumbered by "content" as language is. The basic idea behind such investigations applies not only to language and to music but to all situations where it appears necessary to find objective indices of holistic impressions. How does a connoisseur recognize "style"? How does a physician make a diagnosis? There is a characteristic resistance to these attempts to "objectivize" criteria for decisions, which are often made with remarkable accuracy by specially gifted individuals. Often it turns out that involved calculations of indices yields far less accurate results (with regard to consistency or reliability) than spontaneous guesses, and this "failure" of objective methods seems to give satisfaction to those who depreciate their importance. The comparison is invidious, however, only if one tacitly assumes that the sole purpose of the search for indices is to develop rules of decisions superior to the intuitive bases of decisions. There is, however, another point in these investigations besides a practical diagnostic application: to determine the possibly unconscious basis of the connoisseur's decisions. With this end in view, it becomes highly significant if on the basis of statistical findings one can *approach* the connoisseur's accuracy, for then there is a basis for hypotheses about what sort of data impinges, perhaps subliminally, on the connoisseur to determine his decisions, and it is not at all surprising that the connoisseur does better than the high-speed computer—he probably takes more data into account and makes the computations more rapidly without being aware of them. After all insects are known to make mathematically accurate constructions without the benefits of mathematical training!

It must be admitted, nevertheless, that the search for significant indices unaided by theory is likely to give a very poor yield (recall Edison's search for a suitable filament for the incandescent lamp and Ehrlich's search for a specific against syphilis). Hence excursions into theory are always indicated.

Unfortunately the theoretical sections of Herdan's volume leave much to be desired in clarity and relevance. The exposition of what are presumed to be the fundamentals of information theory will not give a beginner a grasp of the subject matter. Nor can this section serve as a quick review for those who have been exposed to the theory. Rather the obscurities and some outright mistakes are likely to evoke negative reactions in the informed reader. The author does not seem to have a command of information theory, For example he calls the "basic principle of Shannon's theory" the idea of "the suitability of a binary code for replacing a code with any number of elements." This is like saying that the basic principle of arithmetic is that all numbers can be expressed in the decimal system.

Most unsatisfactory is the discussion of

the efficiency of languages as codes. On the one hand it is argued that efficiency demands a proper gradient of frequency distribution among long and short words, the short ones, of course, being most common. This is in accordance with common sense, and the counterpart of this principle in information-theoretical terms is that if the proper gradient is established, the information-per-symbol will be maximized (i.e., redundancy will be minimized). So far so good. But then the argument is turned around, and it is maintained that of the three languages Russian, German, and English, English is the most "efficient" and Russian the least efficient, *because English has the greatest redundancy!* Now it is possible that redundancy enters as a component of efficiency (as combatting noise, for example) but one should not without further elaboration make both arguments at once.

The matter does not end there. It seems that of the three languages, English is capable of the greatest "compression" and Russian is least compressible. This is again cited as evidence of greater "efficiency" of English, even though more "compression" means *less* redudancy!

The greatest confusion reveals itself when the author gives W. Meyer-Eppler's definition of redundancy as the property of the code which enables one to use the probability distribution of the symbols for making valid guesses at the message. However this was meant in the original, the way the definition is applied by Herdan is clearly not in accord with the information-theoretical definition of redundancy, which, however is *also* applied. For consider a cryptogram in which the distribution of the symbols is precisely the same as in English. The cryptogram can be decoded immediately and thus is 100% "redundant" by the definition (as it is applied by Herdan).[4] Does this then mean that English is also 100% redundant?

Another source of confusion is the fast and loose use of metaphorical analogy in interpreting the significance of information.

Much is made of the "principle of duality," supposedly an extension of a well-known mathematical concept. In certain mathematical systems it is possible by properly interchanging certain terms to preserve the validity of the theorems in which they appear. An extension of this concept to the so-called principle of complementarity in quantum physics still enjoys a certain logical validity, since the relations among the terms in physics are expressed with mathematical rigor, allowing the construction of formal identities. However, the author's attempts to extend this principle to linguistics become progressively diffuse until he sees the "principle of duality" operating wherever two terms are juxtaposed. The result gives the impression of a cabalistic "mystique" reminiscent of the writings of German idealist philosophers and of modern extrapolators of fashionable mathematical and physical concepts to all aspects of human affairs. One example will suffice. We quote:

> German is a typical case of retrograde linguistic development. That language which in Goethe and his contemporaries Schopenhauer, Lichtenberg, Humboldt, and later in the Grimm brothers had achieved the highest excellence of precision and flexibility, we have seen in our time under Hitler and his Nazis reach the very depths to which a language can possibly descend . . . .
>
> If we were to interpret what has happened to the German language under Hitler in the light of linguistics, we would say that his work was the obliteration of the fundamental "langue-parole" duality. According to our interpretation of that duality, the spoken or written word represents a sample occurrence of its prototype in "la langue", not only as regards phonemic structure, but also as regards meaning. This is what is meant by "la langue" comprising the word engrams of the speech community. Hitler now utilizing the latent German tendency of being content with the word occurrence itself, and not troubling much about the thought behind it, proceeded in true German thoroughness to sever the connection between "langue" and "parole" completely. . . . Thus was the fundamental duality of language abolished (p. 293–4).

How is this passage to be understood? Just how is the retrograde linguistic development of German demonstrated by the atrocities committed by the Nazis? Yes, there is a branch of language study which purports to analyze the misuse of language for demagogic purposes. The techniques

[4] Possibly this is not what Herdan means by redundancy. Nevertheless the impression remains that a connection is made between efficiency and redundancy via the frequency distributions.

of such misuses are pointed out, for example, in the literature of general semantics. But general semantics, at the extreme *pragmatics* end of the semiotic spectrum, is as far removed as it can possibly be from the sort of linguistics which is the main subject of Herdan's book. The linguistics of phonemes and consonants and *caesurae* and their statistical distributions is at the extreme *syntactical* end of the semiotic spectrum. The building of the bridge to connect syntactics, semantics, and pragmatics is at present a dream of the students of language behavior, a dream which they know full well can be realized only at the cost of enormous toil. It is not altogether right to give the impression that such a bridge already exists. If evidence is on hand showing *how* the German language "retrograded" when Hitler became Chancellor, it should be offered. Otherwise the mixture of scientific terms and indignant utterances detracts from the worth of both linguistic science and of the moral censure of language abuse.

The last part of the book is a straightforward textbook treatment of the elements of statistics.

On the whole *Language as Choice and Chance* can be recommended for its wealth of empirical data (a virtue found also in Zipf's *Principle of Least Effort*, which was severely criticized in the last issue of this journal) but not for the level of its theoretical discussion. One might also suggest a more careful proofreading in subsequent editions, especially of the Russian words in Cyrillic type, which are misspelled so frequently that a reader with knowledge of Russian is likely to be annoyed. For example in a list of eleven Russian authors and titles (p. 116) there are at least 27 mistakes not counting the mixture of old and new orthographies. If, as the author asserts, the redundancy of Russian is low, this "noise" is quite serious. This reviewer thought hard before he recognized Yeyedinsky as Belinsky, while the identity of I. Zren'chrg still remains a mystery to him.

ANATOL RAPOPORT

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Human Components in American Organizations

There is a unique driving force in American organizational processes which has been described with unusual insight and great finesse by a series of political and social analysts from the time of De Toqueville to that of such contemporaries as D. W. Brogan and Alistair Cooke. Anyone who is in a position to compare the American facility to organize with that of any other advanced industrial Western Culture will quickly recognize the existence of an *élan*.

The phenomenon has been described by similes and examples, but now we begin to see attempts to abstract and analyze this quality, subjecting it to sociological scrutiny. What is it in the American national character that permits the ready assemblage of new organization and yet does not encourage an excess of fragmentation? How is it that activities are coordinated to such a fine degree that new highs in organizational performance are established? One way of pin-pointing the responsibility for this capacity is to make systematic comparisons of American organizational patterns with those which come out next best in the range of alternatives offered on the international scene.

One of the neatest comparisons provided in the past few years was made in the steel industry.[1] It has been thirty years or more since steel has been a prestige industry in the United States. It does not show the vigor of petroleum, chemicals, electronics, aircraft, or light metals, but most observers would admit that it is more alert than railroading, trucking, or the construction industry. If anything, the steel industry in the Ruhr has higher relative organizational prestige and would be better able to maintain a proficient labor force in the face of competition. Yet, with less over-all skill and

[1] This material was presented as a seminar to the Committee on Behavioral Science at the University of Chicago in the spring of 1955. Most of the substance will be found in the paper by Harbison, F. H., Cassell, F., Kächling, E., & Ruebmann, H. C. Steel management on two continents. *Management Science*, 1955, 2, 31–39. Further comment will be found in Harbison, F. H. Entrepreneurial organization as a factor in economic development. *Quart. J. Econ.*, 1956, 70, 364–379.

less physical effort—all other factors were found on balance to be as nearly equivalent as they can be made—the American firm outproduced the German by about 2.5 to 1. The difference was to be found in the allocation of duties and responsibilities.

In the American steel production organization a very small fraction of labor force, about 2.5% altogether, was assigned to roles which were not readily comparable to any found in the German organization (or in other parts of Western Europe, for that matter). Upon investigation it was found that these people handled quality control, pilot programs, sales engineering, inventory control, expediting, safety engineering, labor relations, personnel, community relations, internal communications, instrumentation, and many other categories of activities. The age of these persons ranges from the late 20's to the early 40's. They are now coming to be referred to as "organization men." Their duties and behavior characterize, to a far greater extent than will be found for any other class of worker, the special organizational capacities of American culture. In earlier times, the successful managers and their assistants did all of these tasks by themselves (and in Germany that is how they are carried on today) but now they have devolved upon flexible teams of specialists and generalists.

In some organizations, such as advertising or applied research and development, the proportion of "organization men" to the whole roster may rise to 10%, and on rare occasions even to 20%, but they are always a small minority. From coast to coast there are no more than a million such persons at any given time. Yet from looking at them, or tracing their steps for a short while, one would never guess that they epitomize the American genius for organization. Every culture has its hard-driving executives and entrepreneurs, but each must use components which the society makes available to him. These "organization men" enable the American executive to achieve performance standards markedly higher than those achieved elsewhere, and to maintain them with greater consistency. If one is in a position to manipulate these components, he has an unusual and powerful resource.

In his book dealing exclusively with the specifications and characteristics of this kind of person, William H. Whyte has little to say about the significance of "organization man" as a source of power in a competitive world environment.[2] Whyte is more concerned with his private relationships, his morals, education, home life, community participation. More than anything else Whyte is worried about the external sameness of the phylum. He has difficulty breaking it down into species and suspects that this is because it is made up of highly dependent spineless wonders.

Whyte's book is nevertheless an important one. It is filled with gems, large and small, of sparkling insight into contemporary behavior. The last quarter of the book is devoted to a jargon-free, socio-anthropological description of life in a community dominated by organization men and their families which is positively first class.[3]

The argument in Whyte's book proceeds at several levels, and the topmost of these is in the field of ethics. He points out that the formal ideology is that of the Protestant Ethic, but the daily behavior is dominated by quite a different system of values which Whyte calls the Social Ethic. He finds it to be sired by pragmatism, out of scientism. Identification with the organization, its operations and its future, is to be optimized. Whyte calls it "belongingness." Interdependence is also emphasized. This is referred to as "togetherness." It springs from the remarkable groupiness of the present output of the universities. In the Social

[2] Whyte, W. H., Jr. *The Organization Man.* New York: Simon & Schuster, 1956.

[3] I had an opportunity to direct a half dozen advanced graduate students in a quick inquiry into possible futures for Park Forest, the Chicago suburb that Whyte used for his most intensive studies, and also to be aware of the findings of other studies carried on in the same community, both before and after. Whyte was a far better investigator than any of us. He identified social phenomena of suburbia that were missed there and elsewhere by the professionals. My own survey was set up to discover whether there were any contrary indicators (items and trends that might throw a different light upon the matter). His interpretations were backed up at every point. He had neglected nothing that we felt to be important.

Ethic the goals of behavior are virtually beyond question. Its theological debates are concerned with technique—human relations, group dynamics, brainstorming, measurement, and the like.

At another level Whyte traces the life cycle of Organization Man. He is brought through grammar school and high school without trauma. College is, for the largest fraction, a practical preparation for integrative activity in government, business, laboratory, and school. The Organization sets up introductory training programs, so that the transition into productive activity is a painless one. Once inside the man is expected to be loyal and orthodox.

Curiously enough it is still quite common that the unorthodox man is elevated to executive positions. Quite a few of the decisions to be made require manipulation of the organization itself, and sometimes even some surgery. This is a painful experience for organization men who have not been inured to conflict. It is the slightly marginal man who tends to come out on top.

A man is admitted to the Organization through a barrage of psychological tests and is directed to his first posts on the basis of the score he makes. Whyte discerns a tendency for such examinations to be used for determining advancement as well, takes umbrage at the idea, and decides to use craft to subvert the system by providing a pony for those who feel like beating the tests. The bulk of this class of persons, particularly those who fill the nontechnical positions, is much too dependent to use a counterstrategy, but the scientists and engineers are often more ambivalent toward the Organization and are not unwilling to do so.

A scientist or an engineer without an organization is not of much use. He needs a social apparatus for implementing his nontrivial ideas. Yet, once inside, he discovers that the Organization cannot tolerate a large output of new ideas. Either there is insufficient capital to take advantage of them, or too great a risk to current operations, or too little experience with exploitation procedures, or any of a number of other reasons. These logical excuses seldom reach the creators (note the plural, creativity too is nowadays largely a collective proposition), at least not in a credible form, because the grapevine is likely to describe the action in terms of attitudes of cliques and a few powerful personalities. The Organization in America is also exceedingly efficient at burying ideas, saving out only a small fraction for keeping up with the competition. Thus it is not surprising that a large section of the non-university scientists and engineers is bitter. As compared to all other professions, their morale is lowest.

The Organization is no longer parochial, so organization men have become footloose. They are shuffled about from plant to plant, from city to town to metropolis, from home to home. Even after they are permanently attached to headquarters they spend a great deal of time flying from point to point. When they move up into a new salary bracket they are expected to follow suit by buying a new house in the appropriate suburb.

In each new residence the man and his family have learned to put out roots very rapidly. In a single class suburb of home owners (ownership is not an indication of permanency because practically no one expects to keep the house that he now has, so his primary objective in gardening and maintenance is to optimize resale value) this is relatively easy. Within a few months of arrival the man, his wife, and children, find themselves in a whirl of unsophisticated socializing. Kaffee klatsches, PTA's, Scouts, Aqua-Center, church circles, community improvements, and political clubs are typical activities. The often incomplete and inadequate public services of a new and growing suburb are more than made up for by a flurry of voluntary activities. Indeed, because the family sizes are larger, and the strain on the tax rolls for schools, libraries, parks, and tot-lots is so great, one finds a very large share of this activity is expended "for the children." The principal alternative to socializing is television, but even that medium is dominated by the demands of children. They dictate, after school and on until 8–9 P.M., what programs the set will be

tuned to. Organization man carries on some of his community participation "for experience," but mainly it is "for the kids."

The children, it appears, will be even more well-rounded than their parents, knowing a little bit about many things, proficient in popular culture, able to "do it yourself" when reinforced by a group, imbued with the same Social Ethic, that is now sanctioned by a church—almost any denomination—which has adapted to the environment.

If the properties of Organization Man, as described by Whyte, have anything to do with their efficiency then it must be admitted that their children should be even more effective. With such components in plentiful supply the bureaucracies will be still more fluid and adaptive, and the organizations should prosper accordingly. There will be a great number of studies launched in the next few years which are aimed at finding the best blueprint for assembling these components.

I suppose the most disturbing feature of it all is the discovery that the individuals who make up a really top rank organization in America are—at the level where the work gets done—second rate! They have a huge amount of energy, and are packed with information about the current situation, but their horizons are limited and the ideas they express are prosaic. One finds first-rank people in America but they seem to be ejected from the assembly line of the large organization and have been left to fend for themselves. A few, like Whyte in the *Time-Life-Fortune* organization, find a niche which permits them to become relatively independent and nonconforming. Can this become a new trend in social organization?

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More on Cybernetics

An Introduction to Cybernetics. W. Ross Ashby. New York: John Wiley & Sons, 1956.

In this book, Dr. Ashby attempts to cover the major areas of communication and control which have come to be known as cybernetics. Written primarily for workers in the biological sciences, this book should stimulate the interest of a much wider audience. Attitudes toward cybernetics of workers in other fields often go through two stages—an initial enthusiasm, stemming from an impression that at last a new "synthesis" has appeared, followed by a disappointment, stemming from a feeling that old problems have been merely cast into a new vocabulary. Neither position is correct. Cybernetics is indeed a new way of looking at the universe and thus of discovering new relations and constancies. But it does not invalidate all previous schemas nor does it rightly purport to replace Einstein, Newton, Copernicus, or Freud. It should be looked upon as a new tool to be added to our workshops with certain inherent advantages and limitations. Moreover cybernetics is a *mathematical* subject and cannot be expected to be understood without *some* mathematical proficiency.

Ashby seems to feel that a thorough understanding of the most elementary level of that field is necessary for any one who would familiarize himself with the broad problems. Accordingly, he begins by intentionally limiting himself to systems of discrete states, which are treated in great detail and with a wealth of illustrative examples.

Welcome as this limitation is, as a means of giving the beginner a toehold in the theory of systems described in terms of transitions from state to state, the emphasis on discrete states seriously impedes the method by detracting from continuous changes which play a vital part in biology and servomechanisms.

This introduction is followed by a section on information theory containing a good treatment of channel, capacity, and noise. The final section dealing with regulation and control is easily the most stimulating. Although it deals with much the same subject material as the author's earlier *Design for a Brain*, it may be read again with profit.

The book is liberally endowed with problems and examples which unfortunately often assume prior knowledge of biology.

In general, Dr. Ashby has made a noble effort to discuss a subject that is of an inherently mathematical nature in non-mathematical terms. As is often the case in such efforts, the result wavers between a belaboring of the obvious and the glossing over of the difficult. It must be recognized, however, by everyone who has ever attempted writing on the "middle level," i.e., for a sophisticated but unmathematical audience, that this instability of style is practically unavoidable.

All things considered, *An Introduction to Cybernetics* is a welcome addition to the growing informative literature on this subject.

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What is liberty? You say of the locomotive that it runs free. What do you mean? You mean that its parts are so assembled and adjusted that friction is reduced to a minimum, and that it has perfect adjustment. We say of a boat skimming the water with light foot, 'How free she runs,' when we mean, how perfectly she is adjusted to the force of the wind, how perfectly she obeys the great breath out of the heavens that fills her sails. Throw her head up into the wind and see how she will halt and stagger, how every sheet will shiver and her whole frame be shaken, how instantly she is "in irons," in the expressive phrase of the sea. She is free only when you have let her fall off again and have recovered once more her nice adjustment to the forces she must obey and cannot defy.

Human freedom consists in perfect adjustments of human interests and human activities and human energies.

—WOODROW WILSON

ABSTRACTS OF CURRENT LITERATURE WITH INTERDISCIPLINARY IMPLICATIONS

Prepared by Members of the Staff of the Mental Health Research Institute

Kaltofen, Herbert. **Zur Theorie der Nährstoffwirkung.** *Naturwissenschaften*, 1957, 5, 119–20.

The author proposes a general growth equation, applicable to the dry weight of a plant in which the rate of growth is a function of the concentration of nutrient material within the plant. Presumably the latter, having been absorbed through the roots, is expressible as a time integral, whose integrand is proportional to the surface area of the roots.

The result is an integro-differential equation of considerable generality, from which further deductions can be made only if the functions involved are specified. The theory is restricted to those stages of growth when the contribution of nutrient from the seed has become negligible and the process of tissue breakdown has not yet become appreciable. (*AR*)

Lassek, A. M. and Emery, Sara L. **Irreversibility of motor function following bilateral simultaneous cortical lesions.** *Neurology*, 1956, 6, 547.

It has been demonstrated in previous studies that a remarkable recovery of motor function can occur in monkeys after unilateral extirpation of the motor area. This can also be seen if both motor areas are removed in two separate operations. The authors were interested in investigating the effect of bilateral *simultaneous* extirpations of the motor areas. Forty monkeys were used; 15 had total removal of motor cortex only (area 4). In 15 other animals the excision was not limited to area 4 but extended rostrally to involve the premotor or posteriorly to include the postcentral region. The results in these two groups of monkeys were quite similar. Complete motor prostration occurred in most animals. They were unable to sit, stand, walk, run, climb, or eat by themselves. In an additional group of 10 monkeys partial or incomplete lesions were made in area 4. Much less over-all motor deficit and return of function were the rule. All but one possessed the strength to sit up, walk and even climb immediately after the operation. Five of these animals were sacrificed around 64 days postoperatively and the only defects detected at this time were somewhat slower movements and a tendency to tire more easily. It was found at autopsy that restitution of function was possible even when as much as one-half to three-quarters of the posterior portion of area 4 had been removed at one time. Even the performance of skilled movements had returned within two months. It was concluded that the critical region which leads to irreversible loss of motor function is bilateral area 4 down to the corpus striatum. The time factor appears to play an important role in the bilateral ablations. The dynamics of the monkey brain seemed totally disoriented by the loss of the Betz cell area on both sides. (*ER*)

Loeschcke, Hans. **Über die Diffusion von Gas in mit Gas untersättigte Lösungen mit Durchrechnung biologischer Beispiele (On the diffusion of a gas into a solution unsaturated with that gas; with computations referring to biological examples).** *Zt. f. Naturforschung*, 1956, 11 b, 613–620.

Several authors have treated the diffusion of oxygen into tissues when the external supply of oxygen is unlimited. However, so far not much attention has been paid to the case of a gas being exchanged by diffusion between a finite amount of gaseous phase and a surrounding, practically infinite amount of liquid or semisolid phase. A case in point is given by a small air bubble in the blood stream (air embolus). Several

other examples of biological or clinical importance are mentioned; e.g., a pneumothorax, or the absorption of intestinal gases, or also the absorption of gas from the swim bladder of fishes into the surrounding tissues.

The author sets up the equations governing the diffusion process (e.g., for O_2, N_2, or CO_2), and gives approximate and numerical solutions. O_2 consumption in the tissues provides the main difference in total pressure which is responsible for the exchange of gas between the phases.

Two limiting cases of particular interest are: (*a*) diffusion from a gaseous phase at constant volume; the process will continue until all partial pressures are equalized and a true equilibrium is reached. (*b*) diffusion at constant pressure; the gas will eventually leave the cavity at a constant rate, i.e., the diffusion process tends to a steady state. (*ET*)

Pittenger, Robert E., and Smith, Henry L., Jr. **A basis for some contributions of linguistics to psychiatry.** *Psychiatry*, 1957, 20, 61–78.

The authors point up the importance of linguistic tools, and those beginning to be developed in kinesics, for psychiatric diagnosis and treatment, and the development of psychoanalytic theory. "Psychiatric inferences, hypotheses, and theories about the internal as well as the interactional aspects of the person are based largely upon observations of communications." If linguistic procedures are employed in such observations the "area of intuition, hunch, and impression is usefully diminished." Linguistic analysis and kinesic analysis offer an opportunity to describe unambiguously the vocal-kinesic communications of patients and to clarify the differentiating features of the various categories of disturbed persons. Patient-therapist interaction patterns can also be subjected to this sort of analysis, and it is suggested that "some of the causes of subjective experiences become clearer, and it can be hoped that the defensiveness of the therapist can be lessened" with this approach. These points are made in a concluding section.

The major portion of the paper is devoted to a summary of the methods of linguistics. A brief description of (spoken) standard American English is given for illustration of linguistic method and findings. A short section deals with kinesics.

Interpersonal communication is thought of as being made up of three main systems. In order of degree of development they are language (speech), vocal modifiers ("includes all the vocal phenomena—the various kinds and qualities of noises—that are analyzed outside of language proper but relate to language in what may be termed a role of direct support"), and kinesics ("derived from body movement and facial expressions"). Language is described as having two levels—the phonological and the morphological. Stress, pitch and juncture features are treated under morphology. Vocal modifiers are broken down into vocal qualifiers (increasing loudness and increasing softness, raised pitch and lowered pitch, spread register and squeezed register, rasp and openness, drawl and clipping, increased tempo and decreased tempo), vocal differentiators (laughing, crying, breaking), and vocal identifiers.

Voice quality and voice set are not considered to be part of any of the communication systems, but they "seem to be in a close supporting role to language, vocal modifiers, and kinesics." (*MJ*)

Williams, Roger J. **Biochemical individuality.** New York: John Wiley & Sons, 1956.

The author stresses the well-known but all too often neglected fact that a whole host of biochemical variables have wide ranges around the so-called "normal" values. When consideration is taken of the number of such variables, it seems probable that each human being has a biochemical individuality which considerably influences his susceptibility to various diseases, both organic and mental. The implications of such wide distribution patterns of biochemical quantities are explored in a general way. (*JJB*)

About the Authors

Gertrude Falk ("Age and the Nobel Prize") attended Antioch College and the University of Rochester. While a postdoctoral fellow with Dr. R. W. Gerard at the University of Chicago, she took part in the many stimulating group discussions on the conceptual terms and suggestive experimental findings in the behavioral sciences. She states that these discussions "exerted a directive influence on my thinking, but also pointed up the wide gap between theoretical constructs and their translation into experimental terms. This general interest in the way scientific problems are defined and in the experimental approaches used is one of the major reasons the paper on the Nobel prize winners came into being." An assistant professor in pharmacology at the University of Washington School of Medicine, Dr. Falk is interested in the coupling of energetic processes in biological systems and currently is working on the problem of excitation-contraction coupling in muscle.

Frank Harary ("Structural Duality") assistant professor of mathematics and research associate at the Research Center for Group Dynamics, University of Michigan, received his Ph.D. in mathematics from the University of California. His major project at the moment is the writing of a modern comprehensive book on graph theory jointly with Professor R. Z. Norman of Dartmouth College. The only book on graph theory ever written is that of König, which is 21 years old and in German. He feels that "even apart from statistics, mathematics and the behavioral sciences have a great deal to offer each other. The parts of pure mathematics which seem most interesting for this purpose include graph theory, logic and set theory, matrices, and probability theory. On the other hand, those aspects of behavioral science expressible in structural terms appear most amenable to mathematical formulation and development." Harary is spending the year 1957–58 at The Institute for Advanced Study in Princeton, N. J.

Erwin Roy John ("The Acquisition and Application of Information in the Problem-Solving Process") is currently assistant research physiologist at U.C.L.A. Medical College. He is a graduate of the University of Chicago, receiving a B.S. in mathematics and a Ph.D. in physiological psychology. His present research interests center around the effects of tranquilizers on problem solving, effects of injections of chemicals into the brain on learned responses, and changes in the electrical activity of brain structures during learning. His publications include a *Psychological Monograph*, now in press, entitled *Contributions to the study of the problem-solving process.*

Daniel Lerner ("Communication Systems and Social Systems. A Statistical Exploration in History and Policy"), Professor of Sociology at the Massachusetts Institute of Technology, analyzed about 2000 interviews made in seven countries of the Middle East, with the collaboration of Paul Lazarsfeld at Columbia University, and was impressed with (*a*) the restlessness of the "new literates"—those who in each country, having obtained the highly prized secular education, find little or no opportunity to use their new skills and interests, and (*b*) the increasing exposure to the world—through radio and movies—of illiterate and impoverished peasants who, traditionally have neither cared nor shared the interests of the larger humanity. He felt that in these "modernizing" societies-in-a-hurry, a different pace and phasing were at work than those which had guided modern historical developments in the western world. He says, "People there are becoming psychic 'sophisticates'—through exposure to the mass media—before they have any of the usual sociological concomitants such as contacts with urban life, non-farming occupations, money and cash customers, voting and representative government." To see whether "phases" could be fixed, in some precise way, to describe the typical evolution of modern societies, he undertook the study of global correlations between urbanization, literacy, per capita income distribution, mass media, production and consumption, and voting. This article is the first report of a continuing study. Lerner is also the author of four books: *Sykewar: Psychological Warfare Against Germany, Propaganda in War and Crisis, The Nazi Elite,* and *The Policy Sciences,* the latter book with Harold D. Lasswell.

Erik Manniche ("Age and the Nobel Prize") is at present engaged in research on attitude and political behavior study with the Washington Public Opinion Laboratory in Seattle. His major interests are in (a) sociological problems of social control and in (b) history of science and societal factors affecting the general growth and specific directions in the development of scientific knowledge. Born in Copenhagen, he attended the International Peoples College in Elsinore, Denmark, and later the University of Chicago, where he received his Master's degree in sociology. He states that the present paper originated from an interest in factors relevant to the development of science, and also an interest in learning about empirical conditions of various types existing currently in various scientific disciplines. The initial work, he says, may best be described as "idle play concerning the circumstances surrounding the making of particular scientific, mostly theoretical, discoveries; having come across several interesting items we endeavored to systematize the data."

James G. Miller ("The Acquisition and Application of Information in the Problem-Solving Process") is a psychiatrist and psychologist. He is professor of psychiatry and chief of the Mental Health Research Institute at the University of Michigan, and a member of the Board of Editors of *Behavioral Science*.

George Peter Murdock ("Anthropology as a Comparative Science") is currently professor of anthropology at Yale University. His present interests center around comparative research in social organization and the culture history of Africa. The mobilization of the rich resources of ethnography for the development of a general science of human behavior has been a lifelong interest of his. He states that this interest has found expression in the establishment, in 1937, of the Cross-Cultural Survey, now the Human Relations Area Files, and in comparative research on social organization culminating in a volume, *Social structure*, published in 1949. Murdock is also the author of two other books, *Our primitive contemporaries* (1934) and *Outline of world cultures* (1954).

William H. Riker ("Disharmony in Federal Government"), associate professor of political science at Lawrence College, received his doctorate from Harvard University. At present he is particularly interested in federalism and the applications of game theory to politics. He states, "Although students of politics call themselves political *scientists*, there has been very little done in this field that, for example, an economist would call scientific. The mathematical theory of games, which was developed to bring situations of human conflict within the range of mathematical description, gives promise, however, of a genuine science of politics. I am quite excited about this prospect, and the present essay, as well as several others on which I am working, are the product of this excitement." Riker is the author of *Democracy in the United States*, published in 1953, and of *Soldiers of the states: the role of the National Guard in American democracy*, scheduled to be published the latter part of 1957.

Ronald Schaps ("Disharmony in Federal Government") is currently a student at the University of Michigan Law School. He received his B.A. from Lawrence College, and during his senior year there and the following summer he carried out his portion of the work on the present paper.

With this number, the last of Volume 2, of *Behavioral Science* Miss Doris Kenwright, whose duties have been those of managing editor, is leaving the journal to take another position. The Board of Editors has received her resignation with deep regret, and wishes publically to express its thanks and appreciation for her invaluable services during the past two years.

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